ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANIC

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

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ADVERTISEMENT.

THE First Volume of the *Annals of Philosophy* being now before the Public, a better opinion of its plan and execution may be formed than we were capable of communicating by any previous prospectus, or introductory plan. Every Number will be found to include four distinct heads: 1. Memoirs on particular branches of science, either original or foreign, and which are presumed to be unknown to most of the readers of the Journal. 2. An accurate and full analysis of some book of science, chiefly the Transactions published by different Philosophical Societies. 3. Scientific Intelligence. Under this head are included a great variety of important topics, which are, each, too short to constitute a separate essay, but which, notwithstanding, do not constitute the least valuable part of the book. 4. An account of the proceedings of Philosophical Societies. This we conceive to be one of the most valuable departments of our journal. As the accounts are drawn up by the Editor merely from hearing the papers read at the different societies, mistakes and inaccuracies now and then unavoidably occur; but upon the whole he flatters himself that they are tolerably correct, and they must be of value to those numerous readers who have not access to these societies. Care has been taken to confine these reports to societies of first-rate importance. Hitherto the Royal Society, the Linnæan Society, the Wernerian Society, and the French Institute, have alone been noticed; but for the future we intend also to give regularly the reports of the Geological Society, an institution becoming every day of more importance and value, and which will probably contribute most essentially to an accurate knowledge of the structure of Great Britain. We need not notice the List of Patents and the Meteorological Table, both of which are regularly given, and both of which, we flatter ourselves, will be considered as important departments in the Journal.

All these different departments will naturally improve in value as we go on. This, we flatter ourselves, will already appear to any person who will compare the six numbers of the *Annals of Philosophy* already published with each other.

Some complaints have been made that the number of papers in each part is too small; but we flatter ourselves that a little consideration will satisfy the intelligent Gentlemen to whom we allude, that they are as numcrous as is consistent with the size of the part. The average number of papers in each, we find, exceeds 20; so that in the first volume are contained more than 120 different papers, of which a very considerable proportion are original.

It has also been complained that too great a proportion of the *Annals* has been devoted to Chemistry. We admit that, like all other journals of the present day, our *Annals* must contain a greater proportion of Chemistry, which is making a rapid progress, than of those sciences which are in a great measure stationary. But any person who will run over the contents of our volume, will find essays belonging to the following branches of knowledge, namely, Agriculture, Anatomy, Astronomy, Biography, Botany, Geognosy, Hydraulics, Magnetism, Medicine, Meteorology, Mineralogy, Optics, Physiology, Statistics. This comprehends as many different sciences as could well have been expected to make their appearance in one volume, and more indeed than we anticipated in the outset.

We expect every day to receive complete sets of the French and German periodical works for the last three years; which will in all probability furnish us with interesting matter to fill that department of our *Annals* which would otherwise lie vacant during the summer recess of the Philosophical Societies of London.

One other topic it may, perhaps, be proper just to notice. Several letters have been received mentioning the small number of contributions that have been furnished to the *Annals* by the different philosophers of Great Britain, and regretting the great weight that has fallen, in consequence, on the Editor; but the Editor himself is of a different opinion, and acknowledges with pleasure that the number of contributions has greatly exceeded his expectation. He had laid his account with conducting the *Annals of Philosophy* for at least a year without any assistance of much consequence; but he has been agreeably disappointed. He can already reckon among his contributors the names of some of the first scientific men in Britain; and he lies under obligations of an equally important nature to other eminent men, who have not permitted him to make their names public.

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PREFACE.

THE superiority of the moderns over the ancients consists not so much in the extent of their knowledge (though that also is considerable) as in the degree of its diffusion. Among the Greeks, by far the most civilized and philosophical people which antiquity has to boast of, knowledge was confined to the schools, or scarcely ventured to show herself abroad. A few individuals engrossed all the learning of the age, while the great mass of the people were sunk in the most deplorable ignorance. In modern Europe, on the contrary, science is scattered with a much more liberal hand over the whole population. All the upper and middle ranks enjoy the blessing of a liberal education: and in Britain, and some other countries, these constitute a considerable proportion of the people. Improvements in any art, or science, are no sooner made in any country than they are sought for with avidity in every other, and soon make their way over the whole civilized world.

This rapid diffusion of knowledge is no doubt owing to the art of printing, which enables us to multiply copies of books with so much ease: an art to which modern Europe is more indebted for her superiority to former ages than any other. But the immediate instruments employed for the diffusion of scientific and useful knowledge are the periodical publications which exist in such numbers in Britain, France, and Germany, and which make it their professed object to scatter every discovery over the whole extent of their circulation. During the 15th and 16th centuries, when periodical journals did not exist, literary men had no better means of conveying information to one another than epistolary correspondence. And if we look into the voluminous epistles of Erasmus, and of his contemporaries, we may form some idea of the great portion of time which was taken up in this irksome and unprofitable employment; which, after all, could answer the intended purpose but imperfectly, and convey the requisite information to a single individual only, and to the small circle of his friends. An author could only appear before the public, when he had a complete theory to communicate. No single discovery was of sufficient importance to occupy a whole volume. Hence the frequency of two or more individuals being occupied with the same pursuits, publishing on the same subject, and making the same discoveries without any knowledge of what was done by each other. The labours of science were not sufficiently subdivided, and labourer after labourer was apt to move on in the same beaten and unprofitable track. Periodical works save the irksome task of multifarious epistolary correspondence. Every discovery is published as soon as made, fruitless labour is spared, and emulation is kept up and increased by the mutual discoveries of different individuals, by the jarring of opinions, and the clashing of different interests.

The first periodical work of science which made its appearance in Britain was the *Philosophical Transactions*, begun in 1665, and continued for many years, in numbers, published monthly, quarterly, or annually, as materials were more or less copiously supplied. The *Journal des Sçavans* in Paris, and the *Leipsic Acts* in Germany, were somewhat similar in their plan. About the middle of the 18th century the Philosophical Transactions altered their form, and came to be published only in volumes. From that period they have consisted entirely of original papers, and have taken no notice of the discoveries made by foreigners, nor of the scientific books which have made their appearance in different countries. Thus Britain no longer possessed a periodical philosophical journal. The *Monthly and Critical Reviews* indeed had commenced, and were conducted with considerable spirit; but being entirely confined to criticisms on books, they could scarcely be considered as registers of the discoveries in science. Perhaps the *Gentleman's Magazine*, which was conducted for many years with much ability, and which contained a great deal of philosophical as well as miscellaneous information, would have been entitled to rank as a philosophical journal, had not the greater number of its columns been filled with articles of belles lettres and antiquarian research.

The first philosophical journal, exactly similar to several which had already existed for some time on the continent, was begun by Mr. William Nicholson in 1797, under the title of *A Journal of Natural Philosophy, Chemistry, and the Arts.* The editor of this journal was well qualified for the office which he undertook. The journal for several years was excellent, and fully entitled to stand a comparison with any periodical work of the kind which had ever appeared. But, owing probably to the quarto form in which it was for some time published, it never acquired so extensive a circulation as might have been expected from its value.

For some years past, if report says true, it has not been the property of the original editor, but of a bookseller; and in reality edited not by Mr. Nicholson, but by some unknown person employed by the bookseller.

Soon after the commencement of Nicholson's Journal, a rival publication appeared under the name of the *Philosophical Magazine*, edited by Mr. Tulloch, a printer from Glasgow, and publisher of the evening newspaper called the *Star*. It was of a more miscellaneous nature than Nicholson's Journal, and perhaps never contained so much original matter; but its circulation was from the commencement more extensive than that of Nicholson's Journal; and this, unless I am misinformed by the London booksellers, continues at present to be the case.

Besides these two philosophical journals, which perhaps have the most extensive circulation, there are two others of a similar kind published in London; one monthly, the other quarterly; but neither of them, as far as I have observed, contains original papers. The first is the *Repertory of Arts, Manufactures, and Agriculture*. It consists chiefly of the specifications of patents granted for new inventions published without variation from the Patent Office, and contains besides a few additional papers in each number copied from the Philosophical Transactions, or from some other of the British or French scientific journals. The quarterly journal is entitled, *Retrospect of Philosophical, Mechanical, Chemical, and Agricultural Discoveries; being an Abridgement of the Periodical and other Publications, English and Foreign, relative to Arts, Chemistry, Manufactures, Agriculture, and Natural Philosophy*. This, as the title implies, is merely an abridgement of the other three English philosophical journals, of the Transactions published by the different British Societies, and of one or two French periodical works.

Such being the state of the English philosophical journals, our readers will not be surprized that we venture to offer our claims to the attention of the public. We consider the multiplicity of such journals as favourable to the advancement of science in many points of view. It facilitates the publication of useful discoveries, and probably occasions many valuable papers to make their appearance, which in other circumstances would never have been written. It secures the exertions of the respective editors by the powerful feeling of rival interest; and it circulates the valuable dissertations of foreigners through Britain, which might otherwise remain in a great measure unknown to us.

In the present case the editor might easily descant upon the defects of other philosophical journals, he might give a detail of the qualifications necessary for editing such a work with success, he might make a parade of his own attainments, and describe the pains he has taken to secure the occasional contributions of a very wide circle of scientific friends, and to procure the most valuable continental journals with as much rapidity as is consistent with the present limited and decreased state of intercourse with other countries. But many circumstances prevent him from attempting any such enumeration. Whatever pains the editor of a periodical work may take to display his qualifications, and whatever magnificent promises he may make, the public never fail to suspend their judgment, and to decide upon the work by its intrinsic merit. A few Numbers of the ANNALS OF MECHANICAL PHILOSOPHY, CHEMISTRY, AGRICULTURE, AND THE ARTS, will put it in the power of the public to estimate its value, and how far it is likely to contribute to the progress of useful knowledge. It is not necessary to develope the plan which the editor means to follow, any farther than it is developed by the title, and by the number now offered to the attention of the public. It may be necessary, however, to mention, that with regard to plates he does not intend to follow the same regularity as the philosophical journals at present published in London. Should it be necessary on any occasion he will not hesitate to give three or four plates in a single number, and when no plates are wanted in any particular number, he will not scruple to omit them. For he does not consider it as a practice which ought to be followed to introduce papers of trifling value, merely because they require to be illustrated by a plate.

ANNALS

OF

MECHANICAL PHILOSOPHY, CHEMISTRY, AGRICULTURE, AND THE ARTS.

JANUARY, 1813.

Article I.

A Biographical Account of the Honourable Henry Cavendish. By Thomas Thomson, M.D. F.R.S.

THE Hon. Henry Cavendish was born in London, on the 10th of October, 1731. His father was Lord Charles Cavendish, a cadet of the family of Devonshire, one of the oldest houses in England. During his father's lifetime he was kept rather in narrow circumstances. His father allowed him an annuity of 500*l*., and fitted up his stables for his accommodation, where he lived for many years. It was during this period that he acquired those habits of economy, and those singular oddities of character, which he exhibited ever afterwards in so striking a manner. At his father's death he was left a very considerable fortune; and an aunt, who died at a latter period, bequeathed him a very handsome addition. In consequence of the habits of economy which he had acquired, it was not in his power to spend the greater part of his annual income. This occasioned a yearly increase to his capital, till at last it accumulated so much, without any care on his part, that at the period of his death he left behind him nearly £1,300,000, and was the greatest proprietor in the Bank of England. On one occasion, his money in the hands of his bankers accumulated to the amount of £70,000. These gentlemen, thinking it improper to keep so large a sum in their hands, sent one of the partners to wait upon him, in order to learn how he wanted it disposed of. This gentleman was admitted, and, after employing the necessary precautions to a man of Mr. Cavendish's peculiar disposition, stated the circumstance, and begged to know whether it would not be proper to lay out the money. Mr. Cavendish dryly answered, "You may lay it out if you please," and left the room.

Mr. Cavendish hardly ever went into any other society than that of his scientific friends. He never was absent from the weekly dinner of the Royal Society Club, at the Crown and Anchor Tavern. At these dinners, when he happened to be seated near those that he liked, he often conversed a great deal; though at other times he was very silent. He was, likewise, a constant attendant at Sir Joseph Banks's Sunday evening meetings. He had a house in London; which he only visited once or twice a week at stated times, and without ever speaking to the servants. It contained an excellent library, to which he gave all literary men the freest and most unrestrained access. But he lived in a house on Clapham Common, where he scarcely ever received any visitors. His relation, Lord George Cavendish, to whom he left by will the greatest part of his fortune, visited him only once a year; and the visit hardly ever exceeded ten or twelve minutes.

He was shy and bashful, to a degree bordering upon disease. He could not bear any person to be introduced to him, or to be pointed out in any way, as a remarkable man. One Sunday evening, he was standing at Sir Joseph Banks's, in a crowded room, conversing with Mr. Hatchett, when Dr. Ingenhousz, who had a good deal of pomposity of manner, came up, with an Austrian gentleman in his hand, and introduced him formally to Mr. Cavendish. He mentioned the titles and qualifications of his friend at great length, and said that he had been peculiarly anxious to be introduced to a philosopher so profound, and so universally known and celebrated, as Mr. Cavendish. As soon as Dr. Ingenhousz had finished, the Austrian gentleman began, and assured Mr. Cavendish that his principal reason for coming to London was to see and converse with one of the greatest ornaments of the age, and one of the most illustrious philosophers that ever existed. To all these high-flown speeches Mr. Cavendish answered not a word; but stood with his eyes cast down, quite abashed and confounded. At last, spying an opening in the crowd, he darted through it with all the speed he was master of; nor did he stop till he reached his carriage, which drove him directly home.

Mr. Cavendish died on February the 4th, 1810, aged 78 years, four months, and six days. His appearance did not much prepossess strangers in his favour. He was somewhat above the middle size, his body rather thick, and his neck rather short. He stuttered a little in his speech, which gave him an air of awkwardness. His countenance was not strongly marked, so as to indicate the profound abilities which he possessed. This was probably owing to the total absence of all the violent passions. His education seems to have been very complete. He was an excellent mathematician, a profound electrician, and a most acute and ingenious chemist. He never ventured to give an opinion upon any subject, unless he had studied it to the bottom. He appeared before the world first as a chemist, and afterwards as an electrician. The whole of his literary labours consist of 17 papers, published in the Philosophical Transactions, and occupying each only a few pages; but full of the most important discoveries, and the most profound investigations. Of these papers there are ten which treat of chemical subjects, two treat of electricity, two of meteorology, and three are connected with astronomy. Let us take a view of these papers, in the order in which we have mentioned them.

I. *Chemical Papers.*

Mr. Cavendish's first paper was published in the year 1766, when he was 35 years of age. It was entitled, *Experiments on Factitious Air*, and constituted a most important step in the science of chemistry. Dr. Hales had demonstrated that *air* is given out by a vast number of bodies in peculiar circumstances. But he never suspected that any of the airs which he obtained differed from *common air*. Indeed, common air had always been considered as an elementary substance, to which every elastic fluid was referred. Dr. Black had demonstrated that calcareous spar and the mild alkalies differed from quick-lime and the caustic alkalies, in containing a quantity of air, chemically combined with the lime and the alkaline bodies. He called this air *fixed air*; and though he had not examined its properties, there was reason to conclude, from the observations which he made, that *fixed air* was not of the same nature with common air. Mr. Cavendish, in this paper, demonstrates that there are two species of air quite different in their properties from common air. These two are *inflammable air* and *fixed air*. He mentions, likewise, a third species of air; namely, that given out when metals are dissolved in nitrous acid. It differed, as he showed, from the other species, though he did not examine its properties in detail.

The inflammable air (now known by the name of *hydrogen gas*) was obtained by dissolving iron, zinc, or tin, in diluted sulphuric or muriatic acids. Iron yielded about $\frac{1}{22}$ part of its weight of inflammable air, zinc about $\frac{1}{23}$ or $\frac{1}{24}$th of its weight, and tin about $\frac{1}{44}$th of its weight. The properties of the air discharged were the same, whichever of the three metals were used; and whether they were dissolved in sulphuric or muriatic acids. When the sulphuric acid was concentrated, iron and zinc dissolved in it with difficulty, and only by the assistance of heat. The air given out was not inflammable, but consisted of sulphurous acid. These facts induced Mr. Cavendish to conclude, that the inflammable air evolved in the first case consisted of the unaltered phlogiston of the metals, the sulphurous acid of the same phlogiston united to a portion of the acid, which deprived it of its inflammability. He found the specific gravity of his inflammable air about 11 times less than that of common air. This determination is somewhat less than the truth; but the error is probably owing chiefly to the quantity of water held in solution by the air, and which Mr. Cavendish showed amounted to $\frac{1}{9}$th of the weight of the air. He tried the combustibility of the inflammable air when mixed with various proportions of common air, and found that it exploded with the greatest violence when mixed with rather more than its bulk of common air.

Copper, he found, when dissolved in muriatic acid by the assistance of heat, yielded no inflammable air; but an air which lost its elasticity when it came in contact with water. This air, the nature of which Mr. Cavendish did not examine, was muriatic acid gas, the properties of which were soon after investigated by Dr. Priestley.

The fixed air (now known by the name of *carbonic acid gas*), on which Mr. Cavendish made his experiments, was obtained by dissolving marble in muriatic acid. He found that it might be kept over mercury for any length of time, without undergoing any alteration; that it was gradually absorbed by cold water; and that 100 measures of water, of the temperature of 55° absorbed 103·8 measures of fixed air. The whole of the air thus absorbed was separated again by exposing the water to a boiling heat, or by leaving it for some time in an open vessel. Alcohol (the specific gravity of which is not mentioned) dissolved $2\frac{1}{4}$ times its bulk of this air, and olive oil about $\frac{1}{3}$d of its bulk. The specific gravity of fixed air he found 1·57, that of common air being 1·. Fixed air is incapable of supporting combustion; and common air, when mixed with it, supports combustion a much shorter time than when pure. A small wax taper burnt 80″, in a receiver which held 180 ounce measures, when filled with common air only. The same candle burnt 51″ in the same receiver, when filled with a mixture of 1 part of fixed air, and 19 of common air. When the fixed air was $\frac{2}{40}$ of the whole mixture, the candle burnt 23″. When the fixed air was $\frac{1}{10}$th of the whole, it burnt 11″. When the fixed air was $\frac{6}{55}$ or $\frac{1}{9 \cdot 16}$ of the whole mixture, the candle went out immediately.

Mr. Cavendish conceived that the nature of the fixed air given out by marble differed somewhat; or that the elastic fluid emitted consisted of two airs, one more absorbable by water than the other. He drew his conclusion from the circumstance, that after a solution of potash had been exposed to a quantity of fixed air for some time, it ceased to absorb any more; yet if the residual portion of air were thrown away, and new fixed air substituted in its place, it began to absorb again. But Mr. Dalton long after explained this seeming anomaly in a satisfactory manner, by showing that the absorbability of fixed air by water is proportional to its purity; and that when mixed with a great quantity of common air, or any other gas not soluble in water, it ceases to be sensibly absorbed.

Mr. Cavendish ascertained the quantity of fixed air contained in marble, carbonate of ammonia, common pearlashes, and carbonate of potash. But, notwithstanding the great precision with which the experiments were made, these determinations are of comparatively little value; because the proper precautions could not, in that infant state of chemical science, be taken, to have these salts in a state of purity. The following were the results obtained by Mr. Cavendish.

1000	grains of marble contained	408	grains of fixed air.
1000	carb. of ammonia	533	
1000	pearlashes	284	
1000	carb. of potash ..	423	

Carbonate of potash was first obtained in the state of crystals by Dr. Black. Mr. Cavendish formed it by making a solution of pearlashes absorb fixed air till it deposited crystals. He examined the properties of these crystals. They were not altered by exposure to the air, did not deliquesce, and were soluble in about 4 times their weight of cold water.

Dr. M'Bride had already ascertained that vegetable and animal substances yield fixed air by putrefaction and fermentation. Mr. Cavendish found by experiment that sugar, when dissolved in water, and fermented, gives out $\frac{57}{100}$ of its weight of fixed air, possessing exactly the properties of fixed air from marble. During the fermentation no air was absorbed, nor was any change produced upon the common air upon the surface of the fermenting liquor. Apple juice fermented much faster than sugar; but the phenomena were the same, and the fixed air emitted amounted to $\frac{381}{1000}$ of the weight of the solid extract of apples. Gravy and raw meat yield inflammable air during their putrefaction; the former in much greater quantity than the latter. This air, as far as Mr. Cavendish's experiments went, is the same as the inflammable gas from zinc; but its specific gravity he found a little higher.

Mineral waters have at all times attracted the attention of the medical faculty, in consequence of the peculiar properties which they possess, and the medicinal virtues which they are supposed to exhibit. No sooner had the science of chemistry made any progress, than attempts were made by means of it to ascertain to what the peculiar properties of these waters were owing, and to determine the constituents of which they were respectively composed. Some faint attempts towards the analysis of these waters were made by Boyle. Du Clos attempted to make an experimental analysis of the mineral waters in France; and Hierne published a set of experiments on the mineral waters of Sweden. Though these investigations were rude and inaccurate, they led to the knowledge of several facts respecting mineral waters which chemists were unable to explain. One of these, and not the least puzzling, was the existence of a considerable quantity of calcareous earth in some mineral waters, which was precipitated by boiling the water. Nobody could conceive by what means this insoluble substance (*carbonate of lime*) was held in solution; nor why it was thrown down on exposing the water to a boiling heat. It was to determine this point that Mr. Cavendish made his experiments on Rathbone-place water, which were published in the year 1767 (Phil. Trans. Vol. lvii. p. 92); and which may be considered as the first analysis of a mineral water, possessed of tolerable accuracy, ever published. All preceding investigations of this kind, when compared with those of Mr. Cavendish, vanish into nothing. Rathbone-place water was at that time raised by a pump, and supplied the streets of London in its neighbourhood with water. Mr. Cavendish found that when boiled it deposited a quantity of earthy matter, consisting chiefly of lime; but containing also a little magnesia. These, he showed, were held in solution by fixed air; and he proved experimentally that this gas has the property of holding lime and magnesia in solution, when an excess of it is present. Besides these earthy carbonates, Rathbone-place water contained a little volatile alkali, some sulphate of lime, some common salt, and a little sulphate of magnesia. Mr. Cavendish examined, likewise, some other pump water of London; and shewed that they contained lime held in solution by carbonic acid.

Dr. Priestley, at a pretty early period of his chemical career, discovered, that when nitrous gas is mixed with common air over water, a diminution of bulk takes place; that there is a still greater diminution of bulk when oxygen gas is employed instead of common air; and that the diminution is proportional to the quantity of oxygen gas present in the gas mixed with the nitrous gas. This discovery induced him to employ nitrous gas as a test of the quantity of oxygen present in common air. And various instruments were contrived to facilitate the mixture of the gases, and the measurement of the condensation. As the goodness of air, or its fitness to support combustion, and maintain animal life, was conceived to depend upon the proportion of oxygen gas which it contained, these instruments were distinguished by the name of eudiometers. The best of them was contrived by the Abbé Fontana, and is usually distinguished by the name of the *Eudiometer of Fontana*. Philosophers, in examining air by means of this instrument, at various seasons, and in various places, had found considerable differences in the diminution of bulks. Hence they inferred that the proportion of oxygen varied; and to this variation they ascribed the healthiness or noxiousness of particular places. Mr. Cavendish examined this important point with his usual patient industry and acute discernment. He ascertained that the apparent variations were owing to inaccuracies in making the experiment; and that, when the requisite precautions were taken, the proportion of oxygen in air was found constant in all places and at all seasons. He determined, also, by a correct experiment, that air is a mixture of very nearly 21 parts by bulk of oxygen gas, and 79 parts of azotic gas. (Phil. Trans. 1783, Vol. lxxiii. p. 106.)

For many years, it was believed by philosophers, that mercury was essentially liquid, and that no degree of cold was capable of congealing it. Professor Braun's accidental discovery, that it is frozen by cold like other liquids, was at first doubted; and when it was finally established by irrefragable experiments, it was concluded, from the observations of the Petersburgh philosophers, that its freezing point was not less than several hundred degrees below zero. It became an object of great importance to determine the exact point of the congelation of this metal by accurate experiments. This was done at Hudson's Bay by Mr. Hutchins, who followed a set of directions given him by Mr. Cavendish. From these experiments Mr. Cavendish deduced that the freezing point of mercury is very nearly 39° below zero of Fahrenheit's scale. (Phil. Trans. 1783, Vol. lxxiii. p. 303.)

These experiments naturally drew the attention of Mr. Cavendish to the phenomena of freezing, to the action of freezing mixtures, and the congelation of the acids. He employed Mr. M'Nab, who was settled in the neighbourhood of Hudson's Bay, to make requisite experiments. And he published very curious and important papers on these subjects. (Phil. Trans. 1786, Vol. lxxvi. p. 241; and 1788, Vol. lxxviii. p. 166.) He explained the phenomena of congelation exactly according to the theory of Dr. Black, rejecting only the hypothesis that heat is owing to the presence of a peculiar matter, and thinking it more probable, with Sir Isaac Newton, that it is owing to the rapid internal motion of the particles of the hot body. The latent heat of water he found to be 150°. The observations on the congelation of the nitric and sulphuric acids are highly interesting, but not susceptible of abridgement. He showed that their freezing points varied very considerably, according to the strength of each; and drew up tables indicating the freezing point of acids of various degrees of strength. These papers constitute one of the most interesting, and perhaps the best established, part of the theory of heat, as at present taught by chemical philosophers.

But the most splendid and valuable of Mr. Cavendish's chemical experiments were published in two papers, entitled *Experiments on Air*; the first inserted in the Philosophical Transactions for 1784 (Vol. lxxiv, p. 119); and the second in the Transactions for 1785 (Vol. lxxv. p. 372). The object of these experiments was to determine what happened during the *phlogistication of air*, as it was at that time termed; that is, the change which air underwent when metals were calcined in contact with it, when sulphur or phosphorus was burnt in it, and in several similar processes. He showed, in the first place, that there was no reason for supposing that carbonic acid was formed, except when some animal or vegetable substance was present; that when hydrogen gas was burnt in contact with air or oxygen gas, it combined with that gas and formed water; that nitrous gas, by combining with the oxygen of the atmosphere, formed nitrous acid; and that when oxygen and azotic gas are mixed in the requisite proportions, and electric sparks passed through the mixture, they combine and form nitric acid. The first of these opinions occasioned a controversy between Mr. Cavendish and Mr. Kirwan, who had maintained that carbonic acid is always produced when air is phlogisticated. Two papers on this subject are published in the Philosophical Transactions by Mr. Kirwan (Phil. Trans. 1784, Vol. lxxiv. p. 154 and 178); and one by Mr. Cavendish (Ibid. p. 170); each remarkable examples of the peculiar manner of the respective writers. All the arguments of Kirwan are founded on the experiments of others: he displays great reading, and a strong memory; but does not discriminate between the merits of the chemists on whose authority he founds his opinions. Mr. Cavendish, on the other hand, never advances a single opinion which he has not put to the test of experiment; and never suffers himself to go farther than his experiments will warrant. Whatever is not accurately determined by unexceptionable trials, is merely stated as a conjecture, upon which little stress is laid.

In the first of these celebrated papers Mr. Cavendish has drawn a comparison between the phlogistic and antiphlogistic theories; has shown that each of them is capable of explaining the phenomena of chemistry in a satisfactory manner; that it is impossible to demonstrate the truth of either; and he has given the reasons which induced him to prefer the phlogistic theory to the other, which the French chemists were unable to refute, and which they were wise enough not to notice. Nothing can be a more striking proof of the influence of fashion even in science, and of the unwarrantable precipitation with which opinions are rejected or embraced by philosophers, than the total inattention paid by the chemical world to this admirable dissertation. Had Mr. Kirwan adopted the opinions of Mr. Cavendish, when he undertook the defence of phlogiston, instead of trusting to the vague experiments of inaccurate chemists, he would never have been obliged to yield to his French antagonists, and the antiphlogistic theory would never have gained ground.

Such were the chemical papers published by Mr. Cavendish. They contain five notable discoveries: all of them brought nearly to perfection by that illustrious author. These are: 1. The nature and properties of hydrogen gas. 2. The solvent of lime in water when the lime is deposited by boiling. 3. The exact proportion of the constituents of atmospherical air, and the fact that these constituents never sensibly vary. 4. The composition of water. 5. The composition of nitric acid. It is proper to add, that Mr. Cavendish was the first person who showed that potash has a stronger affinity for acids than soda. His experiments on the subject are to be found in a paper on mineral waters published in the Philosophical Transactions by Dr. Donald Monro.

II. *Electrical Papers.*

The papers published by Mr. Cavendish on electricity are only two; but they constitute, perhaps, the most elaborate of all his investigations. His first paper is entitled, *An Attempt to explain some of the Principal Phenomena of Electricity by Means of an Elastic Fluid.* (Phil. Trans. 1771, Vol. lxi. p. 584.) This paper is very long; and contains a very complete mathematical theory of electricity, deduced from the hypothesis that there exists an electric fluid, the particles of which repel each other, but are attracted by all other matter with a force inversely as some less power of the distance than the cube. This paper is not susceptible of abridgement; but it deserves the careful study of every electrician. Æpinus, about the same time, adopted the same hypothesis, and published a book on the same subject, replete with valuable information; but he does not carry the subject quite so far as Mr. Cavendish. Besides the value of this paper in a philosophical point of view, it claims the attention of mathematicians on account of the neatness, simplicity, and shortness of the demonstrations. The only other electrical paper of Mr. Cavendish consists in a set of experiments made on purpose to elucidate the shock communicated by the torpedo, and to show that it was consistent with the known properties of the electric fluid. (Phil. Trans. 1776, Vol. lxvi. p. 196.) This paper is marked by the sagacity and the patient industry which distinguishes every thing that came from the hands of Mr. Cavendish. He succeeded perfectly in the object which he had in view; though the discovery of the galvanic pile has thrown additional light on the nature of the organ by which the torpedo produces that remarkable effect.

III. *Meteorological Papers.*

The meteorological papers avowedly written by Mr. Cavendish are only two; and, perhaps, in strict propriety, the term meteorological ought not to be applied to them. The first of these is an account of the meteorological instruments used at the Royal Society house. (Phil. Trans. 1776, Vol. lxvi. p. 375.) These are the thermometer, barometer, rain-gage, wind-gage, hygrometer, variation compass, and dipping needle. Important observations and instructions are given respecting the proper mode of constructing and using the thermometer, the variation compass, and the dipping needle. Mr. Cavendish's second meteorological paper is a calculation of a remarkable luminous arch, seen Feb. 23, 1784. He shows that its height was not less than 52 statute miles, and that it could not exceed 71 statute miles. (Phil. Trans. 1790, Vol. lxxx. p. 101.) There is strong reason to conclude, from the style, and from the train of observations contained in it, that a report of a committee of the Royal Society, appointed to consider the best method of adjusting the fixed points of thermometers, and of the precautions necessary to be used in making experiments with those instruments (Phil. Trans. 1777, Vol. lxvii. p. 816), was written by Mr Cavendish. But as this is not quite certain, we have not reckoned it among the number of his papers.

IV. *Astronomical Papers.*

The astronomical papers of Mr. Cavendish amount to three; and display the same sagacity and patient industry as his papers on the other departments of science. The first of these papers is on the civil year of the Hindoos, and its divisions; with an account of three Hindoo almanacs, belonging to Charles Wilkins, Esq.; (Phil. Trans. 1792, Vol. lxxxii. p. 383;) the second paper is a letter to Mr. Mendoza y Rios, giving a new rule for finding the longitude by what is called the lunar observations. (Phil. Trans. 1797, Vol. lxxxvii. p. 43.) Mr.

Cavendish's last astronomical paper, and the last paper which he ever published, is an account of a set of experiments made to determine the density of the earth by rendering sensible the attraction of small quantities of matter. (Phil. Trans. 1798, Vol. lxxxviii. p. 469.) The apparatus was originally contrived by Mr. John Mitchel; but was new modelled, and greatly improved, by Mr. Cavendish. The result of these experiments was, that the mean density of the earth does not differ from 5·48 by so much as $\frac{1}{14}$th of the whole; that is, that it is not less than 5·09, nor greater than 5·87. The experiments of Dr. Maskelyne on Schehallion, when corrected by the late observations of Mr. Playfair, give the density of the earth 4·867. These two sets of observations compared, would induce one to suspect that the mean density of the earth does not differ much from 5.

Article II.

On the Oxymuriate of Lime. By Mr. John Dalton.

The article called oxymuriate of lime is of great importance to the manufactures, it being used largely in the bleaching of cotton and linen goods. It would also be of great use to the practical chemist, were its properties generally known. As no book of chemistry that I am acquainted with does any thing more than just mention the article, and as it has lately fallen in my way to investigate its constitution and properties, I thought it might be acceptable to several Members of the Society if the results of my observations were made the subject of a communication.*

The oxymuriate of lime is exhibited in two forms; namely, the fluid, and solid or dry form. In the first case it is made by sending a current of oxymuriatic acid gas into a mixture of lime and water; the mixture is kept in a state of agitation during the process, and the acid combines with the lime, forming with it a compound soluble in water. In the second case, the oxymuriatic acid gas is sent into a vessel containing dry hydrate of lime (that is, lime slacked with the least possible quantity of water); the powder is agitated and the gas combines with it to a certain amount, or till the hydrate of lime becomes saturated. The compound is a soft white powder, possessing little smell. It is partially soluble in water, yielding a solution much the same as that obtained by the former process.

Most salts that are soluble in water are capable of being

* This essay was read at the Literary and Philosophical Society, Manchester, Oct. 2, 1812.

formed again by evaporating the water; either in crystals or in a dry saline mass. This observation does not, however, apply to oxymuriate of lime. Whenever a solution of oxymuriate of lime is evaporated, part of the acid escapes, and the rest is mostly converted into muriatic acid; so that instead of *oxymuriate* of lime, *muriate* of lime is obtained. Hence the dry salt cannot be obtained from the liquid solution. Mr. Tennant, of Glasgow, however, succeeded in obtaining the dry salt in a condensed and portable form, by sending the acid gas into dry hydrate of lime, as stated above.

In whatever state we procure the oxymuriate of lime, it is always found to be accompanied by a portion of muriate of lime; this portion too increases with the age of the oxymuriate, and is furnished at its expence. It becomes a primary object of analysis then to ascertain how much of any given specimen is muriate, and how much oxymuriate; especially as the former is of no use in effecting the purposes for which the latter is applied.

The following experiments are selected out of a very great number which were made, as best calculated to show the properties of the article in question.

EXPER. I.—One hundred grains of recent dry oxymuriate of lime were exposed to a low red heat in an iron spoon. The loss was 32½ grains. To the residuum water was added, and a solution of 535 grain measures of 1·055 sp. gr. was obtained, and further an insoluble residuum of 30 grains. The solution was found to be muriate of lime, and consequently consisted of 16 muriatic acid and 18 lime. The residuum was dissolved in muriatic acid, and formed a solution indicating 21 lime; a small portion of carbonic acid was given off, but not of any amount. No trace of oxymuriatic acid could be found after the salt had been heated.

Hence we learn, that 100 grains of dry oxymuriate of lime contain 39 grains of lime, combined and uncombined; and that by a low red heat, all the oxymuriatic acid is either driven off or converted into muriatic acid.

EXPER. II.—One hundred grains of the same specimen of oxymuriate were added to upwards of 1000 grains of water; after being stirred for some time, the liquid was filtered, and 1000 grain measures were obtained of the sp. gr. 1.034; I got also a residuum which, dried in a moderate heat, gave 33 grains. This last treated with muriatic acid, was dissolved, and indicated 18½ lime. The liquid, which contained a mixture of oxymuriate and muriate of lime, was treated with carbonate of soda, which converted the whole of the lime into carbonate of lime. From the quantity obtained it appeared, that the combined lime in the liquor was also $18\frac{1}{4}$ grains. From this experiment the total quantity of lime in 100 grains of dry oxymuriate was 37 grains. In the former it was 39 grains.

The quantity of lime in the solution being thus found, it remained to find the quantities of muriatic acid and oxymuriatic acid, with which it was combined. The muriatic acid was determined as follows.

Exper. III.—Two hundred grain measures of a solution of the sp. gr. 1·034 were taken; to these a given quantity of muriatic acid test was added, such as previous trials had shown was more than sufficient to expel all the oxymuriatic acid from the lime. The new compound was well agitated in a bottle, and the oxymuriatic acid gas was blown away as long as any continued to be given out.—The liquid solution was then tested, and found to be acid, but not to destroy colour. Nitrate of mercury was then added, as long as any calomel was thrown down. The calomel, when dried, weighed 31 grains; one ninth of this was muriatic acid = 3·44 grains; from this deducting the quantity added to the liquid 2 14 grains;

there remain .... 1·3 grains of muriatic acid previously in combination with the lime. Now we have seen that the lime in 200 measures of liquid was 3·7 grains, which would require 3·5 grains of muriatic acid; it had previously only 1·3 grains; therefore the lime in combination with oxymuriatic acid must have been so much as would require 2·2 grains of muriatic acid to saturate it. Hence it appears that nearly $\frac{1}{3}$ of the lime in the solution was engaged by muriatic acid, and the remaining $\frac{2}{3}$ by oxymuriatic acid. But the quantity of this last was still undetermined.

The usual way of comparing the values of any two bleaching liquids has been, I believe, to find how much of any given coloured liquid a given portion of the acid liquor would saturate. The experiment serves well for the purpose of comparison; but it does not inform us of the precise quantity, either of volume or weight, of the acid gas which the liquor contains. We might expel the acid gas from a given weight, either of the dry or liquid oxymuriate, by means of an acid, in a graduated tube, over mercury or water; but unfortunately both these liquids act upon the acid: no doubt the analysis might be accomplished this way; but it would require an apparatus expressly for the purpose. I have succeeded however another way in discovering a very excellent test of the quantity of oxymuriatic acid in any compound. This test is a solution of green sulphate of iron.—As soon as green sulphate of iron comes in contact with oxymuriatic acid solutions, the black oxide is converted into red, at the expense of the oxygen of the oxymuriatic acid; if the sulphate is deficient, a strong smell of oxymuriatic acid accompanies the mixture; whence more sulphate must be added, till the mixture, on due agitation, ceases to emit the fumes of the oxymuriatic acid: if too much sulphate is put in, then more of the acid liquor must be added by degrees, till its peculiar odour is developed. A very few drops of either liquor are sufficient to give the mixture a character when near the point of saturation. I found that 40 grain measures of a solution of sulphate of iron of the sp. gr. 1·149 were sufficient to saturate 100 measures of oxymuriate of lime of 1·034 sp. gr.—In order to understand more clearly the relative weights of oxymuriatic acid and oxide of iron, which are required for mutual saturation, I made the following experiment.

EXPER. IV.—A graduated tube was filled with oxymuriatic acid gas. This was then plunged into a dilute solution of green sulphate of iron, and the whole of the gas was by due agitation immediately absorbed by the liquid. If any smell of oxymuriatic acid remained, the experiment was repeated on a stronger solution of green sulphate; but if no smell remained, then it was repeated with a weaker solution; till in a few trials the strength of the sulphate was found, which was just sufficient to cover the smell of the gas; or in other words, to saturate the acid. This was when the solution was of 1·0120 sp. gr. or nearly $\frac{1}{12}$ of the strength which I commonly use as a test solution, as mentioned above. Now 100 measures of oxymuriatic acid gas weigh ·29 of a grain, reckoning its specific gravity at 2·46; and 100 measures of the sulphate contain (as I find by experience) 1·32 grain of real dry salt, of which 68 parts are sulphuric acid, and 64 parts oxide of iron; of which 50 are iron, and 14 oxygen, as is well known. The red oxide of iron is known to contain half as much more oxygen as the black; hence 64 parts of black oxide will become 71 of red, or the black oxide receives 7 parts of oxygen from the 29 of oxymuriatic acid, and reduces it to 22 of muriatic acid. These numbers perfectly accord with those deduced as the weights of the respective atoms in the 2d part of my chemistry.—We are now enabled to find the quantity of oxymuriatic acid in the 1·034 oxymuriate of lime. As 100 measures require 40 of 1·149 sulphate of iron, and these contain 3·2 grains of black oxide; we shall have 64 : 29 :: 3·2 : 1·45 grain, for the weight of oxymuriatic acid in 100 measures of liquid oxymuriate of lime of the sp. gr. 1·034. This is five times the volume of the liquid in gas.

In 100 measures of the solution of oxymuriate of lime of 1·034 we therefore find,

1·15	Lime.
·65	Muriatic acid.
1·45	Oxymuriatic acid.
3·25	

But as the lime is united to the acids in separate portions, it may be proper to exhibit the part belonging to each acid, as under:

·65 Muriatic acid ·70 Lime	}	= 1·35	Muriate of lime.
1·45 Oxym. acid . 1·15 Lime	}	= 2·6	Oxym. lime.
		3·95	

It is further observable, that, the oxymuriatic acid and lime are combined in the ratio of 29 to 24 nearly; which shows that the combination is a simple one, or one atom of acid to one of lime.

Returning now to the dry oxymuriate of lime, we find it must be constituted of,

13·5	Muriate of lime.
26	Oxymuriate of lime.
18·5	Lime.
42	Water.
100	

From the manner in which the dry oxymuriate of lime is made, we are compelled to consider the compound as a species of saturation of the lime and acid, and must therefore suppose the whole of the lime (except what is in the state of muriate) to be combined with the oxymuriatic acid. This gives the dry compound as under:

		13·5	Muriate of lime.
14·5 Oxym. acid 30 Lime.....	}	44·5	Oxymur. or suboxymuriate of lime.
		42	Water.
		100	

From this it appears, that the lime is more than sufficient to afford two atoms for one of the oxymuriatic acid. Hence we may infer, that this is the saturation which is produced by the process of making dry oxymuriate of lime; namely, when each atom of the acid is combined with two atoms of lime. So that the dry salt may be denominated the *suboxymuriate of lime.* When dissolved in water, *one half* of the lime is deposited, and a solution of *simple oxymuriate* is obtained.

Age diminishes the value of a solution of oxymuriate of lime, by converting it partially into muriate; but this effect is also produced in degree on the dry salt, when kept in a bottle. I have by me a quantity of the article presented me by Mr. Tennant, the manufacturer, in 1807. It was at first, as nearly as I can ascertain, of the same value as that analysed above.

One hundred grains of this, now five years old, yield a solution of 1000 grains of 1·034 sp. gr., the same as the recent oxymuriate; but the solution only possesses $\frac{1}{4}$ of the oxymuriatic acid which the other does, and gives 58 per cent. lime, combined and uncombined; so that 100 grains of this now, must have been 125 grains originally. It now consists of

30	Muriate of lime.
12	Suboxymuriate of lime.
26	Free lime, with traces of carbonic acid.
32	Water.
100	

By comparing these results with the former, it appears there has been a great diminution of the suboxymuriate, and a great increase of the muriate; but that upon the whole there is a considerable loss of oxymuriatic acid, which is not to be found in the increase of the muriatic acid, and which must therefore have made its escape undecompounded.

Thus we see that the oxymuriate of lime, whether dry or liquid, has a tendency to degenerate into muriate of lime: but it does not appear why so large a proportion should be found in it in the first instance, as $\frac{1}{4}$ or $\frac{1}{3}$ of the whole. I am inclined to think this is accidental, and depends upon the oxymuriatic acid gas not being freed from muriatic acid gas in the original formation of the oxymuriate. That it is not essential, may be proved experimentally, by combining oxymuriatic acid gas with lime in lime water. If the oxymuriatic acid be expelled from the lime immediately, very little muriate of lime is formed. Yet, when either lime water, or simple water, is combined with oxymuriatic acid, a portion of muriatic acid is formed, as is shown by the following experiments.

EXPER. V.—Six hundred measures of lime water took 600 measures of oxymuriatic acid gas = 1.80 grains. Six measures of test nitric acid (as much as was sufficient to saturate the lime) were immediately added, and the oxymuriatic acid was driven off by agitation, &c. Nitrate of mercury was then dropped in, as long as any calomel was precipitated. Five grain measures of the sp. gr. 1·127 were required to saturate the muriatic acid; they contained ·53 gr. mercurial oxide, which would require ·066 of muriatic acid: but the whole oxymuriatic acid contained 1·35 gr.; hence only $\frac{1}{20}$ of the oxymuriatic acid had been converted into muriatic by the process.—600 measures of lime water that had taken its bulk of the acid gas, and been kept about two weeks, required four times as much nitrate of mercury to saturate it, or had become $\frac{1}{5}$ muriate.

Exper. VI.—Six hundred measures of lime water took 600 of oxymuriatic acid gas = 1·80 grain. This was immediately put into a large stoppered bottle, and briskly agitated, the air in the bottle being frequently renewed. When the oxymuriatic acid gas was expelled from the water, nitrate of mercury was dropped in till no precipitate appeared. Five grain measures of 1·127 nitrate were required (the same as in the last experiment) to saturate the muriatic acid. Hence $\frac{1}{20}$ of the oxymuriatic acid had been converted into muriatic, as in the case of lime water: 600 measures of a similar solution, two weeks old, took five times as much nitrate; and hence the acid was become $\frac{1}{4}$ muriatic.

Though it appears from the preceding experiments that the solution of oxymuriate of lime in water exhibits the oxymuriatic acid and lime combined 1 atom with 1 of lime, we are not to consider the lime as holding the maximum quantity of acid in that state. At an early period of my experiments I found that the liquid had all the marks of excess of lime. Indeed, when it is considered that so large a quantity of pure lime is precipitated, we cannot expect the liquid to be neutral. And from the following experiment, it is found that lime in solution can retain more oxymuriatic acid than that above-mentioned.

Exper. VII.—A graduated tube was filled with oxymuriatic acid gas. The gas was absorbed by its bulk of lime water. The compound had no smell of oxymuriatic acid: but if any water containing the acid was added to it, strong fumes were immediately manifest. This shows that lime water can take its bulk of oxymuriatic acid, but no more, so as to neutralize it. Now 100 grains of lime water contain ·12 gr. of lime, and 100 measures of oxymuriatic acid weigh ·29 gr.; hence 24 parts of lime are in this instance combined with 58 of oxymuriatic acid, or 1 atom of lime to 2 of acid. This compound, therefore, is a superoxymuriate, or, as I should rather term it, *binoxymuriate* of lime.

This fact, in conjunction with the observation that free lime is always found in oxymuriate solution, and an hasty experiment from which I inferred that the same quantity of acid was required to neutralize oxymuriate solution as to neutralize the same volume of lime water, conspired for a long time to mislead me respecting the true nature and constitution of liquid oxymuriate of lime. I imagined it was constituted of *muriate of lime*, and *binoxymuriate, dissolved in lime water*; but I always found too much lime, and too little oxymuriatic acid, for this idea. At last I began to suspect that the free lime (as I had conceived it) must be more in quantity than what is found in lime water. By carefully adding dilute acids to a solution, I found that much more acid might be added than was requisite to saturate lime water, without developing the offensive smell of oxymuriatic acid, and without saturating the lime. At length, when *half* of the lime is thus saturated, the other *half* forms a true binoxymuriate with the acid gas, and any additional acid in that case expels the gas in torrents. We have an instance nearly similar to this in the article *phosphate of soda*, as prepared in this country; when in solution it exhibits alkaline properties, and requires as much acid to neutralize it as is sufficient for *half* the soda in the solution; the other half of the soda taking a double proportion of phosphoric acid, and being in that state neutral.

The old oxymuriate of lime being so largely mixed with muriate, I was desirous to know whether they could in part be separated by their different solubility in water. On trial it appeared that the two salts were nearly equally soluble. I obtained a solution of the sp. gr. 1·14, by adding a small portion of water to a large one of the salt; successive portions of water were added, and liquors of various strengths obtained from that above to 1·01. In all these solutions, both muriate and oxymuriate were found; but the oxymuriate was proportionally rather more abundant in the first produce: so that, it should seem, oxymuriate is rather more soluble than muriate of lime; and that they are not to be separated in this mode.

Solutions of oxymuriate of lime absorb nitrous gas rapidly. 100 measures of 1·034 absorb about 270 measures of nitrous gas. The liquid is afterwards acid, and requires about 200 measures of lime water to saturate it. Calculating from the quantity of nitric acid which the nitrous gas should produce, 300 measures of lime water would be requisite to saturate it. Hence it may be concluded, that the 100 measures of oxymuriate of lime solution are, in reality, 100 measures of lime water holding the salts in solution; that is, liquid oxymuriate of lime from the dry salt is, *lime water holding in solution simple oxymuriate and muriate of lime.*

From the experiments I have made by way of double decomposition upon oxymuriate of lime, and the alkaline and earthy salts, I have no doubt that oxymuriatic acid combines with most bases in the same way as with lime; and that a class of salts denominated oxymuriates actually exist, at least in a liquid form. Chenevix, in his paper on oxymuriatic and hyperoxymuriatic acids, seems to doubt of the existence of these salts; and infers from his experiments, that potash and soda are no sooner saturated with oxymuriatic acid gas, than the solutions resolve themselves into simple muriates and hyperoxymuriates. This resolution is undoubted in certain instances, and when the solutions are of great density; but there must be some important circumstances attending these operations, that have yet escaped notice. The oxymuriates of lime and potash are eminently useful in bleaching; but a mixture of the solutions of muriate of potash and hyperoxymuriate would be of no avail in that process. Some further inquiries are evidently requisite on this head, before we can satisfactorily explain the phenomena.

One valuable object, I conceive, obtained by the preceding experiments, is the acquisition of a more complete and easy test of the quantity of oxymuriatic acid in any solution, than we have had hitherto, in green sulphate of iron. It requires little or no skill in the application, and can always be commanded of the same strength; whereas coloured solutions are not easily obtained of the same strength, and are liable to decay. Experiments on the oxidation of metals, and on oxidation in general, will be conducted with greater precision, having a reference to the exact quantity of oxygen with which bodies sublime, when that combination is effected by means of oxymuriatic acid. The quantity of red and green sulphates of iron in given solutions will be readily determined, and the green converted into the red at pleasure. But it is needless to enlarge on the uses and application of the oxymuriates, as they will easily be suggested to the practical chemist when the nature of these compounds becomes more perfectly known.

Article III.

On Ulmin. By Thomas Thomson, M.D. F.R.S.

In the year 1797 Mr. Vauquelin published a paper, entitled *Observations sur une maladie des arbres qui attaque spécialement l'Orme, et qui est analogue a un ulcere.** In this paper he described two kinds of morbid matter which flowed from the common elm; the one whitish, and nearly as limpid as water; the other dark brown, of greater consistency, and covering the bark of the elm with a kind of varnish. The white coloured sanies contained the following substances:

Vegetable matter	0·605
Carbonate of potash	0·342
Carbonate of lime	0·050
Carbonate of magnesia	0·003
	1·000

The brown substance he found a combination of potash and a peculiar vegetable matter, resembling gum in several of its properties, but differing in several circumstances from that vegetable principle. It was soluble in water, insoluble in alcohol, precipitated from its solution by acids, and when burnt yields an acrid smoke, without any smell of caramel.

No notice of these experiments was taken by any subsequent writer. But in the year 1804 Klaproth published a paper, entitled *Chemische Untersuchung eines gummigen pflanzensaftes*

* Annales de Chimie, vol. xxi. p. 39.

von Stamm eines Ulme;* that is, *Chemical experiments on a gummy juice from the stem of an Elm.* The substance on which his experiments were made was sent him from Palermo in 1802; and he conjectures that the species of elm from which it exuded was the *ulmus nigra.* What species he refers to, by the name of *ulmus nigra*, it is difficult to guess; as I am not aware that any such name was ever given by botanists to any species of elm whatever. This substance, according to Klaproth, possessed the following properties.

It was solid, hard, of a black colour, and had considerable lustre. Its powder was brown. It dissolved readily in the mouth, and was insipid. It dissolved speedily in a small quantity of water. The solution was transparent, of a blackish brown colour; and even when very much concentrated by evaporation, was not the least mucilaginous or ropy; nor could it be employed, like mucilage of gum, to paste substances together.

It was completely insoluble both in alcohol and ether. When alcohol was poured into the aqueous solution, the greatest part of the substance precipitated in light brown flocks. The remainder was obtained by evaporation, and was not sensibly soluble in alcohol. The alcohol, by this treatment, acquired a sharpish taste.

When a few drops of nitric acid were added to the aqueous solution, it became gelatinous, lost its blackish brown colour, and a light brown substance precipitated. The whole solution was slowly evaporated to dryness, and the reddish brown powder which remained was heated with alcohol. The alcohol assumed a golden yellow colour; and when evaporated left a light brown, bitter, and sharp resinous substance. Chlorine was found to produce precisely the same effect as nitric acid.

When the exudation from the elm was burnt, it emitted little smoke or flame, and left a spongy, but firm, charcoal; which, when heated sufficiently in the open air, burnt all away, except a little carbonate of potash.

In the third edition of my *System of Chemistry*, I inserted this substance as a peculiar vegetable principle, under the name of ulmin. Though I had some suspicion that it might be the same with the peculiar substance previously discovered by Vauquelin in the diseased exudation from the common elm (*ulmus campestris*); yet, as I had no means of verifying this suspicion, and had no hopes of being able to procure any of the exudation described by Klaproth, I did not venture to hint my suspicion, being apprehensive that it might contribute to increase the confusion of a branch of chemistry by no means remarkable for its precision.

Fortunately, Mr. Walter Coulson, on reading the account of

* Gehlen's Journal, vol. iv. p. 329.

ulmin in my work, recollected having seen a similar exudation from an old elm in the neighbourhood of Plymouth. Conceiving that this exudation might be *ulmin*, he collected a quantity of it, and was so obliging as to send it to me. I seized with avidity an opportunity, quite unlooked for, of putting my conjectures to the test of experiment, and of witnessing the very peculiar properties of ulmin described by Klaproth. The substance which I examined agrees in so many particulars with the properties noticed by Klaproth, that there can be little doubt of its belonging to the same species. The few differences which I observed were probably owing to the different length of time that the substance in question had been exposed to the atmosphere. The substance which I examined being an exudation from the common elm, and agreeing in every particular with the properties noticed by Vauquelin, there can be no hesitation in considering them as similar. Hence it follows, that the vegetable substance first described by Vauquelin, and the ulmin of Klaproth, are one and the same.

The following are the properties of the ulmin from Plymouth, as far as I observed them.

1. It was of a black colour, possessed considerable lustre, and broke with a vitreous fracture. It was nearly tasteless, leaving in the mouth only a very slight impression of astringency. When heated it did not melt, but swelled very much, as is the case with gum. It readily burnt away at the flame of a candle, leaving a white matter, which melted into an opake white bead, and was carbonate of potash. The proportion of this alkali was considerable, agreeing exactly with the exudation examined by Vauquelin. It contained also lime: 20 grains of the ulmin when burnt in a platinum crucible left 5 grains of residue. Of this 4·8 grains dissolved in nitric acid. The 0·2 grain of residue was insoluble, and possessed the properties of silica, tinged a little with iron. The nitric acid solution being saturated with carbonate of potash, one grain of carbonate of lime precipitated. Hence 20 grains of ulmin contained the following substances:

Subcarbonate of potash	3·8
Carbonate of lime	1 0
Silica and oxide of iron	0·2
	5·0

The silica and iron were probably accidentally present, and might have made their way to the ulmin while moist upon the tree; for it is probable that the dust of the road would consist chiefly of silica; or at least would be insoluble in nitric acid, the only criterion by which the 0·2 grains of residue were judged to be silica.

2. It dissolved readily in water. The solution was dark brown,

and possessed exactly the characters described by Klaproth. It produced no effect upon litmus paper, either in its usual blue state, or when reddened by vinegar. Hence the carbonate of potash, which the ulmin contained, must have been in a state of combination.

3. No effect was produced on the solution by isinglass dissolved in water, by tincture of nutgalls, or by prussiate of potash.

4. Green sulphate of iron occasioned a very copious muddy brown precipitate.

5. Muriate of tin occasioned a copious light brown precipitate. The same effect was produced by nitrate of mercury, and superacetate of lead.

6. Nitrate of silver, caustic potash, and carbonate of potash, occasion no precipitate.

7. No precipitate was produced by alcohol, how much soever the solution of ulmin was concentrated. In this respect, my experiments differ from those of Klaproth. It is possible, that if I had employed a stronger alcohol than any I was possessed of when these experiments were made, my result might have corresponded with that of Klaproth. I had not the means of determining its specific gravity. But, as it was procured from an apothecary's shop in London, it was probably not less than 0·837.

8. Nitric acid dropped into the aqueous solution of ulmin occasions a reddish brown precipitate. The liquid being cautiously evaporated to dryness, a reddish matter remains, which is soluble in alcohol, and has a bitter taste. When heated to a temperature between 300° and 400° Fahrenheit, it takes fire even in a close vessel, and burns instantaneously like gunpowder, producing a quantity of gaseous fluid, and leaving a black spongy charcoal behind. This is owing to the nitrate of potash formed by means of the potash contained in the vegetable matter. For when the precipitate is separated by the filter, washed and dried, it loses the property of exploding. When the liquid is gradually evaporated to dryness, prismatic crystals of nitrate of potash shoot at the bottom of the vessel.

9. Sulphuric acid occasions a very copious yellowish brown precipitate when dropped into the aqueous solution of ulmin. Muriatic acid produces the same effect. When this precipitate is well washed and dried, it is a buff-coloured powder, nearly insipid, and not sensibly soluble either in water or alcohol.

From these properties it is obvious, that the characters ascribed to ulmin, by Klaproth, do not apply to the substance which I examined. Ulmin might be compared to extractive; but its insolubility in alcohol seems to make it necessary to constitute it a new genus of vegetable matter.

Article IV.

*Experiments on the Urine discharged in Diabetes Mellitus, with Remarks on that Disease.** By William Henry, M.D. F.R.S.

In the analysis of the urine voided in diabètes, a few circumstances appear not to have been determined with the degree of precision which the subject admits, and which it is desirable to attain; though calculated, perhaps, rather to have an influence on the pathology of the disease, than on its medical treatment. In consequence of the recent occurrence of two cases of diabetes mellitus under my own care, and of other opportunities for which I am indebted to my colleagues† in the Manchester Infirmary, I have lately been enabled to examine several specimens of this variety of morbid urine. The results, I am well aware, do not present any facts of great novelty or importance. Yet they may, perhaps, not be unworthy of being laid before the Society; since they contribute to furnish tests of the existence of the disease, and of the degree in which it is affected by diet or remedies, which are more easily applicable than those hitherto employed. Without entering, therefore, at large into the chemical history of diabetic urine, I shall limit myself to the description of a few of its properties, to which I have particularly directed my attention.

1. *Of the Specific Gravity of Diabetic Urine, and the Proportion of its solid Contents.*

The specific gravity of the urine, discharged in diabetes mellitus, has been left unnoticed by some of the best writers on its chemical history, as Cruickshank, Nicholas and Gueudeville, and Thenard. In about ten cases where I have had an opportunity of determining this property, it has never fallen short of 1028, nor exceeded 1040; 1000 parts of water at 60° Farh. being taken as the standard. This appears to agree very nearly with the experience of the few writers who have noticed its relative weight, and especially of Dr. Bostock,‡ Mr. Dalton, § and Dr. Watt.‖ The circumstance of specific gravity I consider as a most useful test of the existence of diabetes in doubtful cases; and, when the disease is unequivocal, taken along with the actual quantity discharged, it furnishes a good criterion of the degree of morbid action. Healthy urine I have never found,

* Read before the Medical and Chirurgical Society, March 12, 1811.

† Drs. Ferriar, Bardsley, Holme, and Mitchell.

‡ Med. Memoirs, vi. 241.

§ Dr. Bardsley's Med. Reports, p. 161.

‖ Cases of Diabetes, &c. p. 79.

even in its most concocted state (viz. when voided on first rising in the morning), and when an average has been taken of that of several different persons, to have a higher specific gravity than 1020. In the course of the day, also, it falls greatly below that number; while the specific gravity of diabetic urine, though subject to a little variation, never changes during the same day to any thing near the same amount. It may be objected, perhaps, to the employment of this test, that it requires, more familiarity with the method of taking specific gravities, than falls to the lot of the greater part of medical practitioners. By means, however, of an hydrometer, which is well known to practical chemists, and which may readily be procured at a small expense, the specific gravity of the urine may be taken in a few moments, and with the greatest accuracy, by a person wholly unaccustomed to experiments of this kind.*

Respecting the proportion of solid contents, obtainable from diabetic urine, little agreement, as might be expected, is to be found among authors; for besides that the amount actually varies, it must necessarily depend greatly on the degree to which the evaporation is carried. In Captain Meredith's case, described by Mr. Cruickshank,† it appears, at the maximum, to have constituted rather more than $\frac{1}{12}$ of the urine; Dr. Bostock, in a case which he has related in the Memoirs of the Medical Society of London,‡ obtained $\frac{1}{9}$ of a thick syrup; Nicolas and Gueudeville $\frac{1}{14}$ of a mass resembling coarse sugar§; and Thenard from $\frac{1}{17}$ to $\frac{1}{30}$.‖ By this process, it will always be found difficult to obtain an exact comparison between the urine of different persons, or of the same patient at different stages of the disease. It appeared to me, therefore, desirable to connect, by a set of careful experiments, the quantity of extractive matter with the more certain character of specific gravity. From such a series of experiments, I have constructed the following Table, which exhibits, at one view, the quantity of solid matter in diabetic urine of different specific gravities between 1050 and 1020. It will be easy, however, to extend the scale, by the rule of proportion, to any case in which the urine may be found to have a specific gravity above the former, or below the latter, of those two numbers. In the experiments, which furnished the data of the Table, the urine was evaporated by a steam heat, till it

* The hydrometer best adapted to this purpose is made by Mr. W. Twaddell, of Glasgow. To avoid the inconvenient length of the stem, it is divided into four parts; but it is No. 1 only of the series that is required for determining the specific gravity of urine. To reduce the degrees of this instrument to the common standard, the rule is, to multiply by 5, and then to add 1000. Thus 6° of the hydrometer denote a specific gravity of 1030; for $6 \times 5 + 1000 = 1030$.

† In Rollo on Diabetes, 2d edit. p. 19.

‡ Vol. vi. p. 240.

§ Ann. de Chim. xliv. 59.

‖ Ann. de Chim. lix. 47.

ceased to lose weight, and till it left an extract which became quite solid on cooling.

Specific Gravity of the Urine.						
In degrees and tenths of Twaddle's Hydrometer.	Compared with 1000 parts of water at 6° Far.	Quantity of solid extract in a wine pint, in grs. and tenths.	Quantity of solid extract, in a wine pint, in oz.	dr.	scr.	grs.
4.	1020	382.4		6	1	2
4.2	1021	401.6		6	2	1
4.4	1022	420.8		7	0	0
4.6	1023	440.		7	1	0
4.8	1024	459.2		7	1	19
5.	1025	478.4		7	2	18
5.2	1026	497.6	1	0	0	17
5.4	1027	516.8	1	0	1	16
5.6	1028	536.	1	0	2	16
5.8	1029	555.2	1	1	0	15
6.	1030	574.4	1	1	1	14
6.2	1031	593.6	1	1	2	13
6.4	1032	612.8	1	2	0	12
6.6	1033	632.	1	2	1	12
6.8	1034	651.2	1	2	2	11
7.	1035	670.4	1	3	0	10
7.2	1036	689.6	1	3	1	9
7.4	1037	708.8	1	3	2	8
7.6	1038	728.	1	4	0	8
7.8	1039	747.2	1	4	1	7
8.	1040	766.4	1	4	2	6
8.4	1042	804.8	1	5	1	4
8.8	1044	843.2	1	6	0	3
9.2	1046	881.6	1	6	2	1
9.6	1048	920.	1	7	1	0
10.	1050	958.4	1	7	2	18

2. *On the Quantity of Urea contained in Diabetic Urine, with some Inferences respecting the Pathology of the Disease.*

Another circumstance respecting diabetic urine, which has not hitherto been sufficiently determined, is the presence or absence of that substance, the secretion of which is the peculiar office of the kidney; and which gives to healthy urine its characteristic properties. Cruickshank,* Dalton,† Fourcroy,‡ Nicolas and Gueudeville,§ and Thenard,|| have been led to conclude, that urea is not contained, in any proportion whatsoever, in diabetic urine. Dr. Bostock, in the paper already quoted,** expresses a different opinion; but that able philosopher

* Rollo on Diabetes, *passim*.

† Bardsley's Med. Reports, p. 161.

‡ Système des Connais. Chim. 4to. p. 460.

§ Ann. de Chim. lix. 48.

|| Ann. de Chim. xliv. 69.

** P. 260.

was afterwards induced, by farther experiments, to adopt the general belief of the complete absence of urea.*

The test which has hitherto been employed to decide this point, is the addition of nitric acid to the extract of urine dissolved in a small quantity of water. When urea is present, a copious precipitation is immediately produced of bright pearly scales, resembling very nearly in their appearance the acid of borax. And though this test appears to have been considered as somewhat equivocal, from its affording a crystallized substance by its action on sugar, as well as on urea,† yet a little attention will obviate all uncertainty from this source. The change effected by nitric acid on urea takes place at common temperatures; and, when it does not happen immediately, is entirely prevented by heating the mixture, in consequence of the decomposition of a part of the acid by the urea, and the formation of volatile alkali, which unites with the undecomposed acid, and forms nitrate of ammonia. On the other hand, crystals of oxalic acid are never produced, until after the application of a high temperature. The shape of these crystals also is strikingly different from that of the crystals of nitrate of urea; the latter being readily discriminated, by their flat scaly form and pearly lustre, from the crystals of oxalic acid, even when the figure of the latter is modified, as sometimes happens, by the presence of other substances. In some cases, where doubts appear to have existed as to the nature of the product resulting from the action of nitric acid on the extract of urine, I suspect that it has been a mixture of oxalic acid and nitrate of ammonia, both of which have probably been generated, in consequence of the urine having contained urea as well as sugar; a combination not unusual in the less perfect forms of the disease.

In decided cases of diabetes mellitus, it has invariably happened, within my own experience as well as that of other persons, that the nitric acid, applied to the extract of the urine, has failed to give any indications of the presence of urea. There appeared to me, however, reason to suspect, that the action of that acid on the urea might possibly be prevented, by its agency on the greater proportional mass of sugar. To determine this point, nitric acid was added to artificial mixtures of the extract from diabetic and natural urine,‡ with the following results.

Extract from 1 measure diabetic with 1 of natural urine.	The whole presently rendered solid by the abundant precipitation of nitrate of urea.

* Bardsley's Reports, p. 174. † Bostock in Med. Mem. vi. 251.

‡ The diabetic urine was that of S. Brookes, whose case is described by Dr. Ferriar, in his Medical Histories and Reflections, 2d edit. vol. i., p. 135. It had the specific gravity 1033. The natural urine was the portion first voided in the morning by a man in strong health, and had the specific gravity 1019.

Extract from 2 measures diabetic to 1 natural urine.	In the course of a few minutes, a copious precipitation of scales.
Do. from 4 measures diabetic to 1 natural urine.	No immediate precipitation; but it commenced in half an hour and gradually increased.
Do. from 6 diabetic to 1 natural urine.	A very sparing precipitate of pearly scales, but not till after 24 hours.
Do. from 8 diabetic to 1 natural urine.	No scales in 48 hours.

From these experiments, it may be inferred, that urea can no longer be made apparent by nitric acid in the extract, from any mixture of diabetic and natural urine, when the former exceeds the latter in a greater proportion than that of six to one; or, as nearly as I can estimate from other experiments, when the solid urea is less than $\frac{1}{20}$th of the weight of the mixed extract. There is one property, however, of this substance, originally pointed out by Fourcroy and Vauquelin, which enables us to detect urea, even when present in such minute quantities, as to escape discovery by the nitric acid. Amidst the great variety of animal products, this appears to be the only one which is decomposed, when in a state of solution, by the temperature of boiling water. At this low degree of heat, its elements, held together by a balance of affinities which is easily disturbed, arrange themselves in a new order; ammonia and carbonic acid are generated; and carbonate of ammonia is composed, equivalent in weight to about two-thirds that of the urea.* It is in the fluid, therefore, condensed during the evaporation of diabetic urine, that we are to look for traces of the existence of urea; and in this fluid I have invariably found a sufficient quantity of carbonate of ammonia to restore the colour of reddened litmus paper, and to precipitate muriate of lime. When the distillation is carried so far, as to reduce the residuum to charcoal, the last products are strongly acid, in consequence of the production of the pyromucous acid from the de-

* Healthy urine, it is well known, is acid when first voided, and reddens vegetable blue colours, owing, as Thenard asserts, (Ann. de Chim. lix. 270) to its containing acetic acid. After being heated, however, for a short time, the liquor becomes alkaline, in consequence of the production of ammonia. When fresh made urine is distilled, carbonate of ammonia comes over, though in small proportions, till almost the whole of the fluid is evaporated. It is then produced in a great quantity, and lines the neck of the retort and the receiver with a solid incrustation. The quantity of carbonate of ammonia, which I have thus obtained from a wine gallon of fresh and concocted urine, has varied from two to three ounces. Its production may chiefly be referred to the urea, which is equally decomposed by heat and by putrefaction. I have been informed indeed by persons who distil urine for manufacturing purposes, that little if any increase of volatile alkali is gained by previously allowing the urine to become putrid.

composed sugar. Even in these latter products, however, a portion of ammonia exists, and may be obtained in a separate form, by first saturating the liquid with pure potash, and then submitting it to a second distillation. The condensed fluid will invariably be found to contain volatile alkali, though often, it must be acknowledged, in very minute quantity. It is on the ammonia which comes over *early* in the distillation of diabetic urine, that I am disposed chiefly to insist, as establishing the presence of urea; because we are unacquainted with any other animal substance which can give origin to the volatile alkali under such circumstances.

Another proof of the existence of some portion of urea in diabetic urine, may, in many instances, be obtained by a careful observation of the phenomena attending its spontaneous decomposition. At a temperature exceeding 60° Fahr. diabetic urine passes rapidly to the acetous state. But if the succession of changes be carefully watched, it will be found that there is a point at which, before it becomes acid, it exhibits, to sufficiently delicate tests, distinctly alkaline properties.

In the account of these experiments I have not thought it necessary to state the proportion of urea in the fluid submitted to them, because the ingredients of the urine, whether in a healthy or a morbid condition, will scarcely ever be found to have the same proportion to each other. The deficiency of urea in diabetes, however, may be stated as being very considerable. In those cases where I have attempted to estimate it, from the quantity of ammonia evolved by the destructive distillation of the urine, the urea has not appeared to exceed from $\frac{1}{40}$ to $\frac{1}{60}$ the quantity contained in an equal measure of concocted healthy urine. One specimen of diabetic extract, with which I was favoured by Dr. Bardsley, approached so nearly to perfect whiteness, that there appeared to me little reason to expect any evidence of its containing urea. Yet, even in the product of the distillation of this extract, after being rectified with the addition of potash, ammonia was found. On distilling, also, a portion of the urine itself, the condensed liquor gave manifest traces of carbonate of ammonia. This urine, however, contained a far less proportion of urea than I had ever before ascertained; not exceeding, as nearly as I could estimate, $\frac{1}{80}$ of the natural quantity. Making every allowance, then, on account of the increased flow of urine, it will appear that the quantity of urea discharged by persons labouring under diabetes, in any diurnal interval, falls considerably short of the quantity voided in a state of health.

In the examination of diabetic urine, when the disease has not been completely formed, it has occurred to me to find, along with saccharine matter, sufficient urea to give a distinct precipitation on adding nitric acid to the dissolved extract. This is a state of the urine, also, which is often produced by the exclusive use of animal diet. In such cases, I have endeavoured to determine the proportions of the urea and saccharine matter to each other, by the following process: A portion of the extract was first decomposed by destructive distillation; and the product then redistilled with the addition of carbonate of potash. The condensed liquid was next saturated by diluted sulphuric acid of known specific gravity; and from the quantity of this acid which was required, I inferred that of the solid ammoniacal carbonate, every two parts of which were assumed to indicate three of urea. This process I believe to be much more accurate than the treatment of such a mixed extract with nitric acid; because nitrate of ammonia will be formed, and will be mixed with the crystals of oxalic acid, thus rendering their apparent, greater than their real quantity. The precision, however, which is attainable in this way, can only be required in experiments of research. For all practical purposes, the use of the hydrometer, and the application of the test of nitric acid to the extract, will afford a sufficient measure of the degree in which the urine deviates from the healthy standard.

Two hypotheses have been framed to account for the principal phenomena of diabetes. According to the one, the seat of the disease is solely in the organs of assimilation. But it has been satisfactorily proved that saccharine matter does not exist ready formed in the serum of diabetic blood.* Until, therefore, it can be shown that there is a direct communication between the digestive organs and the kidneys or bladder, capable of conveying sugar from the former to the latter without its passing through the general circulation, the theory must be modified by assuming that the blood which reaches the kidneys contains *the elements* of sugar, and is deficient in those of urea. To this theory, however, which takes for granted the healthy state of the kidneys, it may be urged as an objection, that it supposes those glands to have a natural tendency to secrete sugar whenever its elements are presented to them. But this is a point which can scarcely be conceded; for besides that the secretion of urea is known to go on under the exclusive use of vegetable food, such a function in the kidneys would be inconsistent with that wise adaptation of parts, which devotes every organ to some specific purpose essential to the healthy state of the animal economy. It appears, therefore, to be necessary to a just pathology of the disease, that some morbid condition of the kidneys should be admitted, though

* Nicholas and Gueudeville, Ann. de Ch. xliv. 69; Dr. Wollaston, Phil. Mag. xxxvii. 79; and my own experiments, the result of which is stated in Dr. Ferriar's Medical Histories, 2d edit. i. 146. The same conclusion is established, also, by experiments which I have very lately made.

of a kind which has not yet been explained by anatomical investigation.

At the same time, it is probable that the assimilatory organs are also disordered; for the kidneys, though their function is perverted,* so as to render them instruments for forming sugar, still retain, in part, their power of producing urea, when they are furnished with fit materials. This may be deduced from the known influence of animal diet, in diminishing the quantity of urine in diabetes, and in restoring to it that peculiar substance which characterizes healthy urine. In the cases which have fallen under my own treatment, as well as in those which have been shown to me by my medical friends, these have been almost invariable consequences.† But it has not always followed, with equal certainty, as might have been expected from the testimony of some writers, that the disease, in such instances, has been cured. In the first case which I had an opportunity of treating, the urine, in eight days, was reduced from 14 or 16 pints in the 24 hours to 6 pints. Its specific gravity, at the close of that interval, remained the same; but the extract afforded an abundant scaly precipitate with nitric acid. Notwithstanding this change, the strength of the patient, already reduced to an extreme degree by the duration of the disease, sunk so rapidly, that I acquiesced in his wish to return home to a distant part of the country, and to die in the midst of his own family. In this case (and similar ones, I believe, are not uncommon) the kidneys must have regained much of their healthy action, while the general disease remained unsubsided. It should appear, therefore, that neither derangement of the organs of assimilation, nor morbid action of the kidneys, is of itself sufficient to account for the disease: and that both causes are probably concerned in its production.‡

* Dr. Rollo, in his valuable work on Diabetes (p. 418. 427), expresses an opinion that the kidneys are merely *separating but not secreting* organs, adapted to remove excrementitious or unassimilated matter from the system. They appear to me, however, to partake of the office of secretion equally with every other gland in the body; for there is as marked a difference between urea and any of the proximate principles of the blood, as between the latter fluid and the bile, or any other secreted substance.

† One exception only has occurred to me, which I have stated in Dr. Ferriar's Med. Hist. i. 144.

‡ An opportunity has lately occurred to me of trying the plan of treatment in diabetes, which has been recommended, with so strong a body of evidence in its favour, by Dr. Watt, of Glasgow. The patient (a female aged 34) had laboured under the disease more than twelve months, and was then voiding from 12 to 18 pints of urine daily, which had the specific gravity 1037, and gave no traces of urea, except by distillation. Though she was much emaciated, yet her muscular strength did not appear to me to be so far diminished, as to forbid the practice of bloodletting. Between the 28th December and the 14th January, she was bled four times, to the extent of 12 or 14 ounces each time. She was put, also, on a gentle course of mercury, which after some time slightly affected the mouth; and she was laid under no particular restriction as

3. *Of the remaining Ingredients of Diabetic Urine.*

With regard to the action of chemical tests on diabetic urine, and to the nature and proportion of the *saline substances* which it contains, I have nothing to add to the accurate reports which have already been given by Nicholas and Gueudeville, and by Dr. Bostock. With their experiments my own, for the most part, coincide; and tend to establish the conclusion, that the different salts exist in diabetic urine almost in the same proportion to each other as in the healthy state, but that they fall considerably short of the same absolute quantity.

The nature and amount of the *primary animal fluids* (as they have been termed by Dr. Bostock) which are contained in diabetic urine, can scarcely, I apprehend, be determined, till we are in possession of tests which, while they act as precipitants of those fluids, shall have no agency on saline substances. In the present state of this branch of animal chemistry, it unfortunately happens, that all the tests with which we are acquainted afford nothing more than equivocal appearances when applied to the urine. Acetate of lead, for example, is not only precipitated by animal mucus, but by the muriatic and phosphoric salts which abound in that fluid.

It has been disputed whether the *saccharine matter* existing in diabetic urine be identical or not with vegetable sugar. According to Cruickshank,* both substances, if due allowance be made for the saline ingredients of diabetic extract, yield, by the action of nitric acid, very nearly the same proportion of crystals of oxalic acid. Nicholas, and Gueudeville, and Thenard, have obtained, also, by the fermentation of diabetic extract, very nearly the same weight of alcohol, as would result from an equal weight of vegetable sugar.† These circumstances appear to me to be decisive, with respect to the close similarity between the two substances. On the other hand, it has been asserted, that the saccharine matter of diabetic urine cannot, like vegetable sugar, be brought to assume a crystallized form. The absence of this property, however, is not invariable; for I have had an opportunity of observing distinct crystals of sugar, in a portion of diabetic syrup long exposed to the atmosphere. A mouldy scum formed on its surface, which was repeatedly removed and reproduced. In this way much of the animalized matter was

to diet. I did not find, however, that the smallest impression was made either upon the state of the symptoms, the quantity of urine, or its chemical composition; and I, therefore, discontinued the practice. From a solitary case of so hopeless a disease as diabetes, it would be unfair to deduce a condemnation of this or any other plan of treatment.

* Rollo on Diabetes, 2d edit. p. 429. † Ann. de Chim. xliv. and lix.

doubtless separated; and the residuary syrup afforded regular crystals for spontaneous evaporation. Some slight difference may possibly exist between animal and vegetable sugar, but one depending on those minute differences in the proportion of elements, or in their mode of combination; which cannot be appreciated till the analysis of the products of organized bodies has attained far greater refinement and accuracy, than belong to it in its present state.

ARTICLE V.

Chemical Analysis of the Water of the Dead Sea. By Ober Medicinal-Rath Klaproth.*

THE Asphalt lake, known by the name of the Dead Sea, which covers the old ground that, according to Strabo's tale, in consequence of an earthquake, accompanied by frequent eruptions of fire; or, according to the words of the Bible, in consequence of a rain of sulphur, buried the towns of Sodom and Gomorrha, is very remarkable on account of the considerable proportion of salt which it contains. In this respect it surpasses every other known water on the surface of the earth. This great proportion of bitter tasted salts is the reason why neither animal nor plant can live in this water: on which account the name of Dead Sea is applied to it with justice. This great proportion of salt gives to the water so great a specific gravity, that it is capable of bearing weights that would sink in the Ocean. Hence it happens that men, as Strabo long ago informed us, cannot dive in the Dead Sea, but are forcibly suspended upon its surface.

The Dead Sea is farther remarkable on account of the great quantity of asphalt swimming on its surface; which having been originally thrown up from its bottom in a melted state by the agency of subterraneous heat, and being again solidified by the cold of the water, is at last collected on the margin of the lake, and forms an important article of traffic.

(A)

Two different sets of chemical experiments have already made us acquainted with the nature of the salts with which this water is impregnated.

The first of these is that of Macquer, Lavoisier, and Sage, inserted in the Memoirs of the Academy of Sciences for the

* From the Gesellschaft Naturforschender Freund zu Berlin Magazin, 1809, p. 139.

year 1778, and entitled *Analyse de l'Eau du Lac Asphaltite.* Two flasks, sent by the Chevalier Tolesin Guettard, furnished the requisite quantity of water for this analysis.

They found the specific gravity of the water 1·240.

As the result of their analysis, they obtained from 5 pounds of the water 5 ounces of crystallized common salt, but not quite free from a small mixture of the salts with an earthy baze. Farther they obtained 30½ ounces of earthy salts, consisting of four parts of muriate of magnesia, and three parts of muriate of lime. These proportions, reduced to 100 parts, give us the constituents of the salts of the Dead Sea as follows:—

Muriate of magnesia	21·786
Muriate of lime	16·339
Muriate of soda	6·250
	44·375

The second analysis of this water has been published by Dr. Alexander Marcet, in the Philosophical Transactions for 1807, part 2d. The water examined by him, in company with Mr. Tennant, had been brought from the Dead Sea by Messrs. Gordon and Clunis during their travels in the East, and had been sent by them to Sir Joseph Banks.

The specific gravity of this water was 1·211.

From 20 parts of the water there were obtained by evaporating in a sand-bath, at the temperature of 212° Fahrenheit, 7·7 parts of dry saline residue.

As the result of his analysis, he estimates the constituents in 100 parts of the water as follows:—

Muriate of lime	3 792
Muriate of magnesia	10·100
Muriate of soda	10·676
Sulphate of lime	0·054
	24·622

Or, according to another mode of calculating,

Muriate of lime	3·920
Muriate of magnesia	10·246
Muriate of soda	10·360
Sulphate of lime	0·054
	24·580

This estimate does not, however, accord with the original statement, that 20 grains of water leave a residuum of 7· grains of dried salt. To make them agree, 100 grains must have furnished 38½ grains of salt.

(B)

This circumstance, together with the marked difference in the proportions of the salts, furnished by each analysis, induced me to undertake an analysis myself, having been furnished with a sufficient quantity of water for the purpose by Dr. William Thomson, whose recent death, at Palermo, has deprived mineralogy of a zealous disciple. This water had been brought by the Abbé Mariti from the East, and had been given by him to Dr. Targioni Tozzetti.

The water was colourless and transparent, except a small degree of muddiness, obviously owing to the cork stopper. At the bottom of the flasks lay a single cubic crystal, which had again begun to re-dissolve. The taste of the water was bitter, saltish, and sharp. Its specific gravity was 1·245.

Five hundred grains of this water, evaporated to dryness and left upon a sand-bath till they no longer lost any weight, gave as a residue 213 grains of dry salt. This salt, while still warm, was digested with five times its weight of alcohol. After it had been allowed to exert its whole solvent power, by being left in a moderately warm place, and by frequent agitation, the alcohol was decanted off, and the undissolved salt treated again in the same manner with half the quantity of alcohol.

The alcohol was evaporated, and the residual dry salt was again treated with alcohol; but only with a quantity sufficient to take up the most soluble salts, and to separate a portion of common salt which had been dissolved along with them by the alcohol in the first process. The alcohol, being evaporated, left behind 174 grains of a salt mass, consisting of a mixture of muriate of magnesia and muriate of lime.

To determine the proportions of these two salts, the mass was dissolved in water, and precipitated while boiling by carbonate of soda. The edulcorated precipitate was mixed with water, saturated with sulphuric acid, and the liquid was evaporated to dryness. By washing the dry mass with a little water, the sulphate of magnesia was separated from the sulphate of lime, and the magnesia was precipitated at a boiling temperature by carbonate of soda. The precipitated magnesia, which when edulcorated and dried weighed 70 grains, was neutralized with muriatic acid, and the solution evaporated to dryness. The muriate of magnesia, thus restored, was found to weigh, while still warm, 121 grains. By subtracting this quantity from the original 179 grains, we obtain 53 grains as the weight of the muriate of lime.

The muriate of soda, freed by means of alcohol from the salts soluble in that liquid, weighed, after being well dried, 38 grains. But we may reckon 39 grains, the grain of difference wanting to make up the sum total of the salts, being obviously owing to the greater degree of dryness given in the last processes than in the first. The muriate of soda was dissolved in water, and tried with carbonate of soda and muriate of barytes. No precipitation ensued: a proof that it contained no sulphate of lime.

In 100 parts of the water brought by the Abbé Mariti from the lake of Asphaltum, or Dead Sea, and examined by me, there were contained, therefore,

Muriate of magnesia	24·20
Muriate of lime	10·60
Muriate of soda	7·80
	42·60

(C)

The result of these experiments approaches that of Macquer, Lavoisier, and Sage. But the analysis of Dr. Marcet is a good deal different, owing in all probability to the complicated processes and calculations which he followed.

The specific gravity of the water, as stated by the French chemists, agrees likewise very nearly with mine. The sum of the saline ingredients, as stated by these gentlemen, exceeds what I obtained by $1\frac{3}{4}$ grains. This was probably owing to their being in a less degree of dryness; for it is well known, that the two earthy muriates absorb water from the atmosphere while cooling.

The somewhat smaller specific gravity found by Dr. Marcet renders it probable that the water which he examined was collected not far from the place where one of the streams of the river Jordan falls into the Dead Sea.

To give an example of the difference of the ingredients of this water from those of the ocean, I make choice of the specimen of sea water which Sparrman drew in the month of July, 1776, in the latitude of the Canary Islands, from a depth of 60 fathoms, and which Bergman analysed. He found its specific gravity 1·0289; and a Swedish kanne = 100 Swedish cubic inches gave him

Muriate of soda	1393 grains
Muriate of magnesia	380
Sulphate of lime	45
	1818*

The principal difference between the water of the ocean and that of the Dead Sea, consists in this remarkable circumstance, that in the latter the earthy muriates, which give the water its great sharpness and bitterness, exceed the proportion of common salt $4\frac{1}{2}$ times; while, on the contrary, the common salt exceeds the others nearly as much in the water of the ocean.

* Bergman's Opusc. Vol. i. p. 181.

ARTICLE VI.

*TABLE of the Population of Great Britain, according to the Returns made to Parliament in 1811.**

ENGLAND.

Counties of	Population In 1700.	Population In 1750.	Population In 1801.	Population In 1811.	Area in Square Miles.	Annual Proportion. One Baptism to	Annual Proportion. One Burial to	Annual Proportion. One Marriage to
						Persons.		
Bedford	48,500	53,900	65,500	72,600	430	32	56	126
Berks	74,700	92,700	112,800	122,300	744	34	53	144
Buckingham	80,[illegible]00	90,700	111,000	121,600	748	33	49	129
Cambridge	76,000	72,000	92,300	104,500	686	30	44	127
Chester	107,000	131,600	198,100	234,600	1,017	33	50	131
Cornwall	105,800	135,000	194,500	223,900	1,407	32	62	141
Cumberland	62,300	86,900	121,100	138,300	1,497	35	54	138
Derby	93,800	109,500	166,500	191,700	1,077	33	56	137
Devon	248,200	272,200	354,400	396,100	2,488	33	58	113
Dorset	90,000	96,400	119,100	128,900	1,129	35	57	135
Durham	95,5[illegible]0	135,000	165,700	183,600	1,040	33	50	128
Essex	159,200	167,800	234,000	260,900	1,525	33	44	128
Gloucester	155,[illegible]00	207,800	259,100	295,100	1,122	36	61	120
Hereford	60,900	74,100	92,100	97,300	971	36	58	150
Hertford	70,500	86,500	100,800	115,400	602	34	55	163
Huntingdon	34,700	32,500	38,800	43,700	345	31	48	129
Kent	153,800	190,000	317,800	385,600	1,462	30	41	118
Lancaster	166,200	297,400	695,100	856,000	1,806	29	48	108
Leicester	80,000	95,000	134,400	155,100	816	36	57	130
Lincoln	180,000	160,200	215,500	245,900	2,787	32	51	126
Middlesex	624,200	641,500	845,400	985,100	295	40	36	94
Monmouth	39,700	40,600	47,100	64,200	516	47	64	153
Norfolk	210,200	215,100	282,400	301,800	2,013	30	50	128
Northampton	119,500	123,300	136,100	146,100	965	35	52	133
Northumberland	118,000	141,700	162,300	177,900	1,809	37	53	137
Nottingham	65,200	77,600	145,000	168,400	774	32	52	119
Oxford	79,000	92,400	113,200	123,200	742	34	55	138
Rutland	16,600	13,800	16,900	17,000	200	32	53	147
Salop (Shropshire)	101,600	130,300	172,200	200,800	1,403	36	57	143
Somerset	195,900	224,500	282,800	313,300	1,549	35	52	129
Southampton (Hampshire)	118,700	137,500	226,900	253,300	1,533	31	49	106
Stafford	117,200	160,000	247,100	304,000	1,196	32	52	121
Suffolk	152,700	156,800	217,400	242,900	1,566	31	53	128
Surrey	154,900	207,100	278,000	334,700	811	36	45	130
Sussex	91,400	107,400	164,600	196,500	1,461	30	55	129
Warwick	96,600	140,000	215,100	236,400	984	35	42	116
Westmoreland	28,600	36,300	48,000	47,500	722	31	54	135
Wilts	153,906	168,[illegible]00	191,200	200,300	1,283	35	54	136
Worcester	88,200	108,000	143,900	165,900	674	32	52	132
York, East Riding	96,200	85,500	144,000	173,000	1,268	30	47	115
Do. North Riding	98,600	117,200	160,500	157,600	2,112	30	51	135
Do. West Riding	236,700	361,500	582,700	675,100	2,633	31	51	123
Totals	5,108,500	6,017,700	8,609,000	9,855,400	50,210	33	49	120

* For the sake of comparison, the population in 1700, 1750, and 1801, is also given.

WALES.

Counties of	Population In 1700.	In 1750.	In 1801.	In 1811.	Area in Square Miles.	Annual Proportions. One Baptism in	One Burial to	One Marriage to
						Persons.		
Anglesey	22,800	26,900	35,000	38,300	402	38	72	139
Brecon	27,200	29,400	32,700	39,000	731	35	54	129
Cardigan	25,300	32,000	44,100	5[illegible],000	726	41	[illegible]	141
Carmarthen	49,700	62,000	69,600	79,800	926	42	6[illegible]	131
Carnarvon	24,800	36,200	43,000	51,000	775	35	67	137
Denbigh	39,700	46,900	62,400	66,400	731	33	52	140
Flint	19,500	29,700	41,000	48,100	309	31	5[illegible]	154
Glamorgan	49,700	55,200	74,000	88,000	829	37	53	1[illegible]1
Merioneth	23,800	30,900	30,500	32,000	691	40	62	129
Montgomery	27,400	37,000	49,300	53,700	982	33	62	152
Pembroke	41,300	44,800	58,200	62,700	575	41	6[illegible]	135
Radnor	15,300	19,200	19,700	21,600	455	36	56	144
Totals	366,500	449,300	559,000	632,600	8,125	37	60	122

SCOTLAND.

Counties of	In 1700.	In 1750.	In 1801.	In 1811.	Area in Square Miles.
Aberdeen	....	....	....	135,075	
Argyll	....	....	....	85,585	
Ayr	....	....	....	103,954	
Banff	....	....	....	36,668	
Berwick	....	....	....	30,779	
Bute	....	....	....	12,033	
Caithness	....	....	....	23,419	
Clackmannan	....	....	....	12,010	
Dumbarton	....	....	....	24,189	
Dumfries	....	....	....	62,960	
Edinburgh	....	....	....	148,607	
Elgin	....	....	....	28,108	
Fife	....	....	....	101,272	
Forfar	....	....	....	107,264	
Haddington	....	....	....	31,164	
Inverness	....	....	....	78,336	
Kincardine	....	....	....	27,439	
Kinross	....	....	....	7,245	
Kirkudbright	....	....	....	33,684	
Lanark	....	....	....	191,752	
Linlithgow	....	....	....	19,451	
Nairn	....	....	....	8,251	
Orkney and	....	....	....	23,238	
Shetland	....	....	....	22,915	
Peebles	....	....	....	9,935	
Perth	....	....	....	135,093	
Renfrew	....	....	....	92,596	
Ross & Cromartie	....	....	....	60,853	
Roxburgh	....	....	....	37,230	
Selkirk	....	....	....	5,889	
Stirling	....	....	....	58,174	
Sutherland	....	....	....	23,629	
Wigtown	....	....	....	26,891	
Totals	1,048,000	1,403,000	1,652,100	1,805,688	29,1[illegible]7
Ditto, allowing proportion in army & navy	....	....	....	1,865,000	

GRAND TOTALS.

England	....	....	....	9,855,400	50,210
Wales	....	....	....	632,600	8,125
Scotland	....	....	....	1,865,000	29,167
				12,3[illegible]0	87,502

Second Summary (taking the Army and Navy separately).

ENGLAND.	Males	4,575,763
	Females	4,963,064
WALES.	Males	291,633
	Females	320,155
SCOTLAND.	Males	826,191
	Females	979,497
Army, Navy, Marines, and Seamen, in registered vessels		640,500
	Grand Total	12,596,803

MALES in	England	4,575,763
	Wales	291,633
	Scotland	826,191
	Army and Navy, &c.	640,500
	Total	6,334,087

FEMALES in	England	4,963,064
	Wales	320,155
	Scotland	979,497
	Total	6,262,716

	HOUSES.				OCCUPATIONS.		
	Inhabited.	By how many families occupied.	Building.	Uninhabited.	Families chiefly employed in agriculture.	Do chiefly employed in trade, manufactures, and handicrafts.	Do. not comprehended in the two preceding classes.
England	1,678,106	2,012,391	15,188	47,925	697,353	923,588	391,450
Wales	119,398	129,756	1,019	3,095	72,846	36,044	20,866
Scotland	304,09	402,068	2,341	11,329	125,799	169,417	106,858
Totals ..	2,101,597	2,544,215	18,548	62,349	895,998	1,129,049	519,168

FAMILIES in	England	2,012,391
	Scotland	402,068
	Wales	129,756
		2,544,215

This gives us rather more than 4¼ persons to each family.

Article VII.

Report of a Select Committee of the House of Commons on Transportation.

(As ordered to be printed July 10, 1812.)

The Committee appointed to inquire into the manner in which Sentences of Transportation are executed, and the Effects which have been produced by that mode of Punishment; and who were empowered to report their Observations, and the Minutes of Evidence taken before them, to the House;—— have agreed upon the following Report.

The principal settlement on the eastern coast of New South Wales, was formed in 1788. It is situated in latitude 33 south, longitude 170 east. The most considerable district is that of Sydney, containing, by the return dated the 1st of March 1810, 6,158 inhabitants. Paramatta contains 1,807; Hawkesbury, 2,389; and Newcastle, 100. Of the total number 10,454, 5,513 are men, 2,220 women, and 2,721 children. Of these, from $\frac{1}{4}$ to $\frac{1}{3}$ are convicts; but the returns of their number have been so irregular, that your committee have not been able precisely to ascertain it. But they hope that this neglect will be corrected by the orders lately sent out from this country. The troops are about 1,100 in number, and the remainder are free persons. In addition to these, are the settlements of Port Dalrymple and Hobart's Town, in Van Diemen's Land, about 5 degrees to the south of Sydney; containing 1,321 inhabitants; and at the date of the last returns, 177 persons were living in Norfolk Island, but orders have been since sent out for its total abandonment. The settlement in New South Wales is bounded on the north west and south by a ridge of hills, known by the name of the Blue Mountains, beyond which, no one has yet been able to penetrate the country; some have with difficulty been as far as 100 miles in the interior; but beyond 60 miles, it appears to be no where practicable for agricultural purposes; and, in many places, the diameter of the habitable country is much less: in length, it extends from port Stephens to port Jervis, comprising from north to south about 4 degrees; beyond these, it is stated, that the colony will not be capable of extension; and of the land within these boundaries, about one-half is said to be absolutely barren. The ground actually in cultivation, amounts to rather more than 21,000 acres, and 74,000 acres are held in pasture. The stock appears to be considerable: by the return in 1810, the amount was,—horses, 521; mares, 593; bulls, 193; cows, 6351; oxen, 4,732; sheep, 33,818; goats, 1,732; hogs, 8,992. Of these, a small proportion is kept by government; of which, part is killed for the supply of the public store, and the remainder is made use of to stock the farms of new settlers. It appears from the evidence, that the colony has for some years, except when the crops have failed, from inundations or other accidental causes, been able wholly to supply itself with corn; but that it is still necessary to continue, to a certain extent, the importation of salted provisions. The soil and climate are described to be extremely fine, healthy, and productive; diseases, with the exception of such as arise from intemperance or accident, are little known; and fresh fruits and vegetables are produced from the beginning to the end of the year. The river Hawkesbury is however occasionally subject to violent and sudden floods, which have in some instances totally destroyed the produce of the farms in its vicinity, upon which the colony principally depends for its subsistence. Great difficulties have in consequence at times occurred; and though precautions are now taken to remove the crops as soon as possible from the low grounds in the neighbourhood of the river, no perfect security is yet obtained against the recurrence of these disasters. The out-settlements of Port Dalrymple and Hobart's Town in Van Diemen's Land, are represented as enjoying a purer climate and more generally productive soil than New South Wales, and to be otherwise prosperous and thriving. Yet your committee must concur in the opinion already expressed by his Majesty's government, that more benefit to the colony will be derived from the cultivation and improvement of the settlements that are already formed, than from the formation of new and distant establishments, whatever may be the encouragement that a fertile soil or an advantageous situation may appear to hold out.

The currency of the colony consists principally of government paper and copper money, but from its scarcity, many of the transactions which in other countries would be accomplished by money, are here carried on by barter; thus the labourer is not paid in money but in kind: he demands from his employer such articles as he is most in need of, and they are delivered to him at the prices which they bear in the market. At times indeed wheat and cattle have in the courts of justice been considered as legal tender in payment of debts. To remedy these inconveniencies, a supply of silver coin, to the amount of 10,000*l.* has lately been sent to the colony: but whilst the necessity of large importations continues, with the restraints upon exportation, it is not likely that this coin will long remain there in circulation. The exportations from the colony have hitherto principally consisted of oil, seal-skins, coals, and wool; the fisheries appear to have been much neglected; and the iron ore, of which there is abundance, and of very fine quality, has not yet been worked. The trade in skins and coals is the most thriving, but is much straightened by the restrictions in favour of the East India Company. The stock of sheep is not yet sufficiently large to make wool an article of large exportation. The culture of hemp has been less attended to than might have been expected; a profitable trade in sandal wood has at times been, though illegally, carried on with the South Sea Islands and China; woollen manufactories, potteries, and breweries have been established, but not with any great success. The commercial regulations of the colony have in many instances been so impolitic as much to discourage mercantile speculation; for many years a maximum price was imposed by the Governor upon all imported merchandize: and at this price, often too low to afford a fair profit to the trader, the whole cargo was distributed amongst the civil and military officers of the settlement, who alone had liberty to purchase; and articles of the first necessity were afterwards retailed by them, at an enormous profit, to the poorer settlers. Part of these abuses were corrected in the year 1800; but in the traffic of spirituous liquors, they continued to a very late period, and it is therefore with the greatest satisfaction that your committee have learnt that measures have been enforced, as well by the government here as in the colony, to put an end to these practices. It is stated in a dispatch from Governor Macquarie, dated April 30, 1810, that every care will be taken to prevent the officers of the 73d regiment, now in New South Wales, from resorting to any low or unmilitary occupations, either mercantile or agricultural, for additional means of support; and he justly adds, that such pursuits and avocations are subversive of all military discipline, and incompatible with the rank and character of officers in his Majesty's service. And it is to be hoped that means will also have been devised to restrain the civil officers from making, as has been too often the case, the authority of their stations the means of promoting their own mercenary views. It will be for the executive government to consider how far the memorial of the officers praying for an increase of pay, in consequence of the deprivation of these emoluments, is to be attended to. But it does not appear to your committee that the military officer is in New South Wales exposed to such hardship, or obliged to incur such expense, as to entitle him to benefits not generally bestowed upon officers of the British army. Your committee have also learnt with satisfaction, that many of the improper restrictions, by which commercial speculation has been thwarted in the colony, have been put an end to. The imposition of a maximum price upon all imported articles of merchandise has been discontinued. The maximum on the price of grain and butcher's meat is no longer in existence; and though a similar limit to the price of labour was formerly frequently attempted, it has been, as might be expected, always either evaded or disregarded. One commercial regulation appears however to be still in full force, which, in the opinion of your committee, ought immediately to be rescinded; it is that by which no ship is allowed to dispose of any merchandise in Van Diemen's land, unless it shall have previously touched at Fort Jackson. By this restriction all mercantile enterprise is at once put an end to in the dependent settlements; and supplies, absolutely essential to the support of its inhabitants, may in their greatest necessity be delayed to them. The impolicy and injustice of this regulation are so apparent, that your committee trust it will not long remain in existence.

The greatest difficulties to which the government has been subject, have arisen in its attempts to regulate the supply of spirituous liquors. Their importation used to be limited by licences granted by the Governor: on the arrival of a cargo, he fixed the price at which it was to be sold, and distributed it at this price, which was generally very low, to the persons highest in authority in the settlement. The liquors were afterwards paid away by them as wages to their labourers, or retailed at a very advanced rate to such of the inhabitants as wished to become purchasers; and the eagerness for spirituous liquors has been so great in the colony, that the gains made in this traffic have been enormous. The temptations too to smuggling and illicit distillation are so great, and their facilities in that thinly inhabited country so numerous, that all attempts to check a clandestine supply have proved in vain; and the qualities of the liquor thus obtained, are generally infinitely worse and more unwholesome than of that which is legally imported. Governor Macquarie states in his dispatch, dated April 30, 1810, that the various measures that have been hitherto taken to check the importation and regulate the sale of spirits have invariably failed; and as it is impossible totally to suppress the use of them, a certain quantity being essentially necessary for the accommodation of the inhabitants, it appears to him that it would be good and sound policy to sanction the free importation of good spirits, under a high duty of not less than three or four shillings per gallon. He expects, from this measure, to put an end to all further attempts at monopoly, and bartering spirits for corn and necessaries, and to private stills, which, in defiance of every precaution, are still very numerous in the colony; and he is persuaded that this measure, instead of promoting drunkenness and idleness, will tend rather to lessen both. Governor Macquarie's suggestion met with the approbation of the government of this country, and orders were sent out to permit the free importation of spirits, under a duty of not less than four shillings per gallon. Upon this subject your committee entirely agree with Governor Macquarie in opinion, that a less limited supply of spirituous liquors will not give that encouragement to idleness and inebriety which, at the first view of the subject, naturally presents itself as an objection to the extended importation: it has been stated before them repeatedly in evidence, that the scarcity of spirits has had no other effect than to stimulate the avidity with which they were sought; and that in times when the supply has been most regular and abundant, drunkenness has been the least prevalent. But they are of opinion, that an unlimited supply of spirits may be furnished to the colony in a manner much more conducive to its interests than by permitting a free importation. The want of an extended corn-market, where the prices are regulated by a fair and liberal competition, is much felt in the colony. Of 10,452 inhabitants, 4,277 are wholly, or in great part, victualled from the public store; and three-fifths of the corn brought to market are purchased by the Governor, at a price, over which, from the largeness of his demand, he has always a power of controul, and which many governors have taken upon themselves absolutely to limit, so as scarcely to afford to the farmers a fair profit for their produce; and in the distant parts of the settlement, they have been known to feed their pigs with the corn for which they could not obtain a sufficient price. From the occasional overflowing of the Hawkesbury, and consequent scarcities, a larger cultivation of corn than is necessary for the mere annual subsistence of the colony, is extremely desirable: and your committee are of opinion, that an enlarged market, great encouragement to agriculture, and a free supply of spirits, may be afforded to the colony, without losing the revenue which would be produced by the duties on importation, if distillation within the colony were permitted under proper duties and regulations; and they confidently suggest, that this is a measure which ought to be substituted for that proposed by Governor Macquarie: it would extend agricultural speculation,—it would be a resource in times of scarcity, and, with proper attention, would afford a better spirit than has been hitherto imported; for the importations have, for the most part, been of Bengal and American rum. But your committee must at the same time regret, that an impediment has arisen to the immediate alteration of the present system, from a contract entered into by Governor Macquarie, under which certain merchants have agreed to build an hospital for the settlement, on being allowed, during the ensuing three years, exclusively to purchase spirits at the government price, no other spirits being permitted, within that time, to be imported into the colony by private individuals.

The courts of judicature are instituted by commission; the civil court is called the Court of Civil Jurisdiction, and consists of the Judge Advocate, and two respectable inhabitants of the colony, to be from time to time appointed by the Governor; and they have full power to hear and determine, in a summary way, "all manner of personal pleas whatsoever:" they have also full power to grant probates of wills, and administration of the personal estates of intestates dying within the settlement; and if either party find him, her, or themselves aggrieved by any judgment or decree given or pronounced by the said Court, he, she, or they shall and may appeal to the Governor; or in case of his absence or death, to the Lieutenant Governor; and if any party shall find him, her, or themselves aggrieved by the judgment or determination of the said Governor, in any case where the debt or thing in demand shall exceed the value of 300*l.* such party so aggrieved may appeal to the King in Council. An allowance to be fixed at the discretion of the Court, is to be made by all complainants, at whose suit any person shall be imprisoned, to such defendants, provided such defendant make oath that he has no estate or effect sufficient to maintain himself. It is in evidence that this allowance has been fixed at 6*d.* per day, which is not more in value than 4*d.* in this country. It appears also, that no convict can, during the time of his servitude, sue or be sued in these courts, or suffer imprisonment for any debt incurred by him. By the same commission, a Court of Criminal Jurisdiction is established, upon the authority of the Act 27 Geo. III. c. 2. It is a court of record, with all such powers as are incident to courts of record in England: it consists of the judge advocate and six officers of the sea and land service, to be convened from time to time by precept, under the hand and seal of the Governor, with power (having taken the oaths directed in the commission) to hear and determine all crimes committed within the settlement, and to cause punishment to be inflicted according to the laws of England, as nearly as may be, considering and allowing for the circumstances and situation of the place and settlement aforesaid, and the inhabitants thereof. The verdict to be by the opinion of the major part of the court: if the offence be capital, the court may pronounce judgment of death, or of any punishment not amounting to death, which to the court shall seem meet. But unless five of the persons sitting in the court shall concur in the verdict, execution of any judgment of death shall not be had or done until the proceedings shall have been transmitted to this country, and the pleasure of the King had thereon; and in no capital case is the sentence to be executed without the consent of the Governor, who has power to suspend such execution until he have the direction of his Majesty. Your committee have to observe, that all the evidence examined on the subject, unequivocally condemns the manner in which the criminal courts are thus established. Governor Bligh having stated that they consisted principally of military officers, proceeds—"It did not give satisfaction to the "inhabitants,—they were particularly desirous that they might "not be so much in the power of the military, but might have "some kind of justice that might bring them nearer to their "brethren in Great Britain." He also states that there were settlers sufficient in character and numbers to furnish juries; and thinks their decisions would have been fairer than those that took place without them. Similar to his, are the opinions of Governor Hunter, Mr. Palmer, and Mr. Campbell, and upon their evidence your committee are of opinion, that the manner of administering criminal justice may be altered with great advantage to the colony. It is not to be expected that its inhabitants should view, otherwise than with jealousy and discontent, a system which resembles rather a court martial than the mode of trial, the advantages of which they have been accustomed to see and to enjoy in their own country. However necessary it may have been, at the first foundation of the settlement, thus to constitute the courts, as well from the default of other members as from the refractory habits of the persons then composing the colony; that necessity has now ceased to exist; a numerous class of respectable persons is now formed within the settlement, amply sufficient to warrant the establishment of that trial by jury, for which they are anxiously wishing. But upon this subject your committee particularly refer to a memorial of Mr. Bent, the judge advocate, whose views have met with the approbation of Governor Macquarie; in which the inconveniences of the present system are most ably and clearly detailed, and such regulations are suggested, as appear to your committee to be most worthy the attention and consideration of the government. It is stated in the evidence, that the natives of the country find the same protection in these courts of justice with the subjects of the King. Yet your committee have observed with surprise, in a report of the prisoners tried before the Court of Criminal Jurisdiction in March 1810, that a person charged with shooting at and wounding a native, was tried simply for an assault, whilst another who had committed a similar offence against an European, was tried on the same day for his life. It appears proper to your committee here to remark, that great inconveniences are felt in the colonies in Van Diemen's Land, from the want of a court of justice. The jurisdiction of the magistrates is all that the inhabitants have to look to for their protection against offenders; and for the settlement of civil differences, they have no power within the colony of appealing to the law; all causes and great offences are removed for trial to Port Jackson, to an inconvenience and expense too manifest to need any remark; a Judge Advocate is already appointed, and the additional expense to be incurred by the complete formation of a court,

adapted to the male population of that colony, would not be great. The commission and instructions under which the Governor acts, are given at length in the appendix. He is made Governor and Captain General, with the most enlarged powers, uncontrolled by any council, with authority to pardon all offences (treason and murder excepted), to impose duties, to grant lands, and to issue colonial regulations. It is in evidence from Governor Bligh, that to the breach of some of these regulations, issued at the sole will of the Governor, a punishment of 500 lashes is annexed, and to others a fine of 100*l*. The manner in which these extensive powers have been used, has not always been such as to give satisfaction to the colony; nor can it be expected where so much authority and responsibility are thrown into the hands of one man, that his will however just, and his administration however wise, will not at times create opposition and discontent amongst men unused, in their own country, to see so great a monopoly of power. Under this impression, your committee think it right to recommend, that a council be given to the Governor, for the purpose of sharing with him in the responsibility of the measures which they may think necessary for the security or prosperity of the colony. It may perhaps be doubted how far it will be wise to limit the authority of the Governor over a colony in which, more than any other, the government ought to be strong and unfettered; but the views of your committee would to some degree be obtained, even though the council appointed had no other power than that of protesting against any measures of the Governor of which they might disapprove; and of transmitting their protests to the Secretary of State. The acquiescence of the council would give popularity to the measures of which it approved, and its expressed disapprobation might have the effect of checking such as were evidently inexpedient.

The Governor has the power of making grants of land; and your committee have heard with surprise, that this power has, in one instance at least, been used in a manner, to say the least of it, liable to much observation. It has been stated in evidence, that a grant of land to the amount of 1,000 acres, was made by a Governor, to the person appointed to succeed him, who, immediately on assuming the government, made a similar grant to his predecessor. Upon this your committee must suggest, that no Governor ought on his own account to enter into farming speculations; his salary ought to be sufficient to support him; he ought never to look to other and indirect means of enriching himself.

For many years the Governor was restrained from granting longer leases within the town of Sydney, than for the period of 14 years. This impolitic regulation, which caused much dis-

BALÆNA MYSTICETUS, or *Great Northern Whale.*

Engraved by J. Shury for Dr. Thomson's Annals. Published by R. Baldwin Paternoster Row [illegible] content, and materially checked all enterprize in building, has lately been rescinded. Many settlers have been sent out from this country by government, to whom grants of land, sometimes to a large amount, have been made; and in many instances their want of capital, of character, and agricultural knowledge, have exposed them to difficulties on their arrival, and excited complaints against them for misconduct. Your committee are glad to learn that greater precautions are now taken in the selection of these persons than appears formerly to have been the case. None are allowed to go out as free settlers, unless they can prove themselves to be possessed of sufficient property to establish themselves there without the assistance of government, and who can produce the most satisfactory testimonials and recommendations from persons of known respectability; the person allowed to go, is then recommended to the Governor, to whose discretion it is left to make what grant of land he may think expedient. The form of the answer which is given to all applicants, and one of the letters of recommendation, upon the model of which they are generally drawn, are to be found in the appendix. Your committee wish, however, to suggest that it ought to be made a principle, in selecting these persons, to give the preference to those who have been previously accustomed to agricultural pursuits.

(*To be concluded in our next.*)

Article VIII.

*Account of the Balæna Mysticetus, or Great Northern or Greenland Whale.** By Mr. W. Scoresby, jun., M.W.S.

(Illustrated by an Engraving.)

The whale, when full grown, is from 50 to 65 feet in length, and from 30 to 40 in circumference, immediately before the fins.† It is thickest a little behind the fins, and from thence gradually tapers towards the tail, and slightly towards the neck. It is cylindrical from the neck until near about the junction of the tail and body, where it becomes ridged.

The head has a triangular shape. The bones of the head are very porous, and full of a fine kind of oil. When the oil is drained out, the bone is so light as to swim in water. The jaw-

* From the Memoirs of the Wernerian Society, vol. i. p. 578.

† It is said that the whale was formerly much larger than it is at present, being sometimes 100 or 120 feet long: but the accuracy of this statement is to be questioned; for the largest I ever heard of being caught did not exceed 70 feet in length; and this was reckoned a very uncommon individual. Of about 200 which I have seen taken, not one measured 65 feet in length, although many of them were full grown.

bones, the most striking portions of the head, are from 20 to 25 feet in length, are curved, and the space between them is 9 or or 10 feet, by 18 or 20. They give shape to the under part of the head, which is almost perfectly flat, and is about 20 feet in length, by 12 in breadth. The tongue is of great size, and yields a ton or more of oil. The lips, which are at right angles to the flat part of the base of the head, are firm and hard, and yield about two tons of oil.

To the upper jaw is attached the substance called *whalebone*, which is straight in some individuals, and in others convex. The laminæ or *blades* are not all of equal length; neither are the largest exactly in the middle of the series, but somewhat nearer the throat; from this point they become gradually shorter each way. In each side of the mouth are about 200 laminæ of whalebone. They are not perfectly flat; for besides the longitudinal curvature already mentioned, they are curved transversely. The largest laminæ are from 10 to 14 feet in length, very rarely 15 feet in length. The breadth of the largest at the thick ends, or where they are attached to the jaw, is about a foot. The Greenland fishers estimate the size of the whale by the length of the whalebone; where the whalebone is six feet long, then the whale is said to be a *size fish*. In *suckers*, or young whales still under the protection of the mother, the whalebone is only a few inches long. The whalebone is immediately covered by the two under lips, the edges of which, when the mouth is shut, overlap the upper part in a squamous manner.

On the upper part of the head there is a double opening, called the *spout-holes* or *blow-holes*. Their external orifices are like two slits, which do not lie parallel, but form an acute angle with each other. Through these openings the animal breathes.

The eyes are very small, not larger than those of an ox; yet the whale appears to be quick of sight. They are situated about a foot above where the upper and under lips join.

In the whale, the sense of hearing seems to be rather obtuse.

The throat is so narrow as scarcely to admit a hen's egg.

The fins are from 4 to 5 feet broad, and 8 or 10 feet long, and seem only to be used in bearing off their young, in turning, and giving a direction to the velocity produced by the tail.

The tail is horizontal; from 20 to 30 feet in breadth, indented in the middle, and the two lobes pointed and turned outwards. In it lies the whole strength of the animal. By means of the tail, the whale advances itself in the water with greater or less rapidity; if the motion is slow, the tail cuts the water obliquely, like forcing a boat forward by the operation of *skulling*; but if the motion is very rapid, it is effected by an undulating motion of the rump.

The skin in some whales is smooth and shining; in others, it is furrowed, like the water-lines in laid paper, but coarser.

The colour is black, grey, and white, and a tinge of yellow about the lower parts of the head. The back, upper part of the head, most of the belly, the fins, tail, and part of the under jaw, are deep black. The forepart of the under jaw, and a little of the belly, are white, and the junction of the tail with the body grey. Such are the common colours of the adult whale. I have seen piebald whales. Such whales as are below size, are almost entirely of a bluish-black colour. The skin of suckers is of a pale bluish colour. The cuticle, or scarf-skin, is no thicker than parchment; the true skin is from three-fourths to an inch in thickness all over the body.

Immediately beneath the skin lies the *blubber*, or fat, from 10 to 20 inches in thickness, varying in different parts of the body, as well as in different individuals. The colour, also, is not always the same, being white, red, and yellow; and it also varies in denseness. It is principally for the blubber that the Greenland fishery is carried on. It is cut from the body in large lumps, and carried on board the ship, and then cut into smaller pieces. The fleshy parts and skin connected with the blubber are next separated from it, and it is again cut into such pieces as will admit of its being passed into casks by the bung-hole, which is only three or four inches in diameter. In these casks it is conveyed home, where it is boiled in vessels capable of containing from three to six tons, for the purpose of extracting the oil from the *fritters*, which are tendinous fibres, running in various directions, and containing the oil, or rather connecting together the cellular substance which contains it. These fibres are finest next the skin, thinnest in the middle, and coarsest near the flesh.

The annexed table shows the quantity of oil a whale of each size of bone will produce at a medium.

Bone in Feet.	Oil in Tons.
1	1½
2	3
3	3½
4	4
5	4½
6	5½
7	7
8	9
9	11
10	13
11	16
12	20

The blubber of a sucker, when very young, frequently contains little or no oil, but only a kind of milky fluid; in which case, when the animal is deprived of life, the body sinks to the bottom, as also does the blubber when separated from it; while the body and blubber of larger individuals always swim. Though the preceding statement be exceedingly near the truth, yet exceptions occur; for I have known a whale of $2\frac{1}{2}$ feet bone produce 10 tons of oil, and one of 12 feet bone estimated at only 9 tons; such instances are much rarer than to see one of $2\frac{1}{2}$ feet bone produce 4 or 5 tons of oil.

The flesh of the young whale is of a fine red colour; that of the old approaches to black, and is coarse, like that of a bull, and is said to be dry and lean when boiled, because there is but little fat intermixed with the flesh.

The following are the dimensions of two different whales, taken with accuracy:—

	Ft.	Inch.
First. The longest laminæ of whalebone	10	10
Full length	51	
Length of the head	16	
Diameter of the body	6	
Behind the fins	9 or 10	
Length of the fin	7	
Breadth	4	
Breadth of the tail	20	
Depth from the indented part, where the two lobes join, to the junction with the body	4	
Diameter of the body at its junction with the tail, perpendicularly	2¼	
And horizontally	2	
Second. The largest laminæ of the whalebone	11¼	
Extreme length	58	
Circumference just behind the fins	30 or 35	
Length of the under-part of the head	19	
Breadth	12	
Length of the jaw-bone	23	
Length of the fin	9	
Breadth of the tail	24	

Thickness of the blubber at a medium 9 or 10 inches, and of a red colour.

Estimated to produce 19 or 20 tons of oil.

The food of the whale is generally supposed to consist of different kinds of sepiæ, medusæ, or the clio limacina of Linnæus; but I have great reason to believe, that it is chiefly, if not altogether, of the squillæ or shrimp tribe; for, on examining the stomach of one of large size, nothing else was found in it; they were about half an inch long, semi-transparent, and of a pale red colour. I also found a great quantity in the mouth of another, having been apparently vomited by it. When the whale feeds, it swims with considerable velocity under water, with its mouth wide open; the water enters by the fore-part, but is poured out again at the sides, and the food is entangled and sifted as it were by the whalebone, which does not allow any thing to escape.

It seldom remains longer below the surface than twenty or thirty minutes; when it comes up again to blow, it will perhaps remain ten, twenty, or thirty minutes at the surface of the water, when nothing disturbs it. In calm weather, it sometimes sleeps in this situation. It sometimes ascends with so much force, as to leap entirely out of the water; when swimming at its greatest velocity, it moves at the rate of seven to nine miles an hour.

Its maternal affection deserves notice. The young one is frequently struck for the sake of its mother, which will soon come up close by it, encourage it to swim off, assist it, by taking it under its fin, and seldom deserts it while life remains. It is then very dangerous to approach, as she loses all regard for her own safety in anxiety for the preservation of her cub, dashing about most violently, and not dreading to rise even amidst the boats. Except, however, when the whale has young to proteet, the male is in general more active and dangerous than the female, especially males of about nine feet bone.

The principal enemies of the whale are the sword-fish and thrasher. It is probable that the shark is also an enemy to the whale, for it attacks the dead carcase; and the whale is seen to fly those quarters of the sea where the shark abounds.

Hitherto no accurate representation of this vast animal has appeared in the writings of zoologists. The drawing here engraved I executed in Greenland, and its accuracy was proved, by finding that it agreed in every particular with the numerous individuals I afterwards met with in the Arctic Ocean.

Article IX.

Analyses of Books.

Philosophical Transactions of the Royal Society of London for the year 1812.—Part I. This part contains the following papers:—

I. *On the Grounds of the Method which Laplace has given in the Second Chapter of the Third Book of his Mecanique Celeste, for computing the Attraction of Spheroids of every Description.* By James Ivory, A. M.

II. *On the Attractions of an extensive Class of Spheroids.* By J. Ivory, A. M.

These two papers are, perhaps, the most profound which have appeared in Britain on any mathematical subject for many years; and would be sufficient alone, though the author had done nothing else, to raise him to the rank of one of the first mathematicians of the age. Uncommon pains have been taken to render these papers as perspicuous as possible: but it seems scarcely possible that they should be understood without a previous acquaintance with the Mecanique Celeste of Laplace.

III. *An Account of some Peculiarities in the Structure of the Organ of Hearing in the Balæna Mysticetus of Linnæus.* By Edward Home, Esq. F.R.S. The most remarkable of these peculiarities are the muscular structure of the membrana tympani, its distance from the bones of the ear, and a membranous ligament which connects it with the malleus.

IV. *Chemical Researches on the Blood, and some other animal Fluids.* By William Thomas Brande, Esq. F.R.S. In this paper Mr. Brande gives an account of his experiments on chyle, lymph, and blood. Chyle he found white, it coagulated spontaneously, the coagulum resembled the curdy part of milk. When the serous part of chyle was evaporated, a sweet tasted salt was obtained, which Mr. Brande thought similar to the sugar of milk. Lymph differs but little from pure water. Mr. Brande has shown that blood contains no gelatine; and he has given proofs, which appear decisive, that the colouring matter of blood is not iron, as the French chemists have supposed, but a substance of an animal nature, which may even be employed as a useful article in dyeing.

V. *Observations of a Comet, with Remarks on the Construction of its different Parts.* By William Herschel, LL.D. F.R.S.

VI. *On a gaseous Compound of Carbonic Oxide and Chlorine.* By John Davy, Esq. The discovery of this gas, to which Mr. Davy has given the name of *phosgene gas* (because it is produced only by the action of light), is one of the beneficial consequences resulting from the controversy between Mr. Davy and Mr. Murray, respecting the composition of chlorine. Phosgene gas may be formed by mixing equal bulks of chlorine gas and carbonic oxide gas, both as dry as possible, in a glass vessel well exhausted, and perfectly air tight. If this mixture be exposed to the light of the sun, the two gases speedily combine, and are condensed into half their bulk. Even the light of day is sufficient to produce the combination in about twelve hours. Phosgene gas possesses the following properties:—

1. It is colourless, has a strong and exceedingly disagreeable smell. Its specific gravity is 3·474, that of common air being 1·000: 100 cubic inches of it weigh 105·97 grains. It is, therefore, the heaviest gaseous body known. When thrown into the air it does not smoke.

2. It reddens vegetable blues.

3. It combines with ammonia, and condenses four times its bulk of that gas. The result is a neutral salt, having a pungent saline taste, deliquescing in the air, and very soluble in water. Decomposed by sulphuric, nitric, and phosphoric acids, and likewise by liquid muriatic acid; but sublimed, unaltered in muriatic, carbonic, and sulphurous acid gases. It dissolves without effervescence in acetic acid. The products of its decom-

position collected over mercury were carbonic and muriatic acid gases,* and the volume of the latter was double that of the former.

4. Phosgene gas is decomposed by water, carbonic acid and muriatic acid being formed.

5. Tin fused in the gas decomposes it; fuming liquor of Libavius is formed; and carbonic oxide, equal in bulk to the phosgene gas, remains. Zinc, antimony, and arsenic, heated in the gas, produce the same effect. White oxide of zinc, and protoxide of antimony, likewise, decompose it.

6. Sulphur and phosphorus sublimed in the gas produced no change.

7. Mixed with hydrogen or oxygen singly, the mixture was not inflamed by the electric spark; but when the two gases were mixed at once, in the proportion of two measures of hydrogen and one of oxygen, a violent explosion was produced by electricity, and muriatic and carbonic acid gases were formed.

VII. *A Narrative of the Eruption of a Volcano in the Sea, off the Island of St. Michael.* By S. Tillard, Esq. Captain in the Royal Navy. Captain Tillard, approaching the island of St. Michael, one of the Azores, on the 12th of June, 1811, in the sloop Sabrina, observed columns of smoke rising at a distance, which were at first conceived to arise from two ships engaged at sea. But it was soon observed to proceed from a volcano, which had just broken out in the neighbourhood of St. Michael. Captain Tillard and some other gentlemen next day crossed the island of St. Michael, and witnessed the appearances of the volcano for several hours. It was within a short mile of St. Michael, and in a part of the sea formerly 25 fathoms deep. While they surveyed it, a peak elevated itself above the sea, and became very conspicuous before they left the place. About a month after, the effects of the volcano had subsided; and an island was formed, somewhat less than a mile in circumference. Upon this island, Captain Tillard, and some of his officers, landed with difficulty, and named it Sabrina. It consisted chiefly of a narrow steep cliff surrounding a crater. This crater was full of sea water, boiling hot, and it emptied itself by the east side into the sea.

VIII. *On the Primitive Crystals of Carbonate of Lime, Bitter Spar, and Iron Spar.* By William Hyde Wollaston, M. D. Sec. R. S. These three minerals, which differ essentially in their composition; the first being composed of carbonic acid and lime; the second of carbonic acid, lime, and magnesia; the third of carbonic acid and oxide of iron; had been conceived by mineralogists to agree in their primitive form: and this agree-

* This statement is somewhat obscure. We should have been told how the decomposition was produced. It could not be by heat, because the salt sublimes without decomposition. It was probably by an acid which combined with the base and let the phosgene gas at liberty.

ment had been urged as an objection to the theory of crystallization as delivered by Haüy. Dr. Wollaston, by means of his very ingenious and valuable goniometer, has ascertained that the primitive figures of these three bodies, though nearly similar, differ somewhat in the measurement of the angles of each. The angle of carbonate of lime is ....105° 5′
bitter spar ....106° 15′
carbonate of iron ....107°

IX. *Observations intended to show that the Progressive Motion of Snakes is partly performed by means of the Ribs.* By Everard Home, Esq. F.R.S. Sir Everard Home in this paper describes the progressive motion of serpents, which is performed by means of the ribs, which act as feet. The paper is interesting, as it makes us acquainted with a new species of progressive motion never before suspected.

X. *An Account of some Experiments on the Combinations of different Metals and Chlorine, &c.* By John Davy, Esq. In a science entirely dependent on experiment, and of so vast an extent as chemistry, every new view is attended with considerable advantage, by leading to experiments upon substances that had formerly been neglected. Sir Humphrey Davy's new opinion respecting the nature of chlorine, affords a striking example of this. While this substance was considered as a compound of muriatic acid and oxygen, the effects of it upon other bodies were easily deduced theoretically. Hence few attempts were made to examine them. But when it came to be considered as a simple substance, the case was very different. The compounds which it formed would probably be new, and they merited examination. Mr. Davy was induced to make his experiments in consequence of these views; and in this paper he has made us acquainted with many substances, some of which are new, while others had been already known under other names. Sir Humphrey Davy's opinion respecting the nature of chlorine, is, we presume, known to most of our readers. He considers it as an undecompounded substance, and as analogous to oxygen in many of its properties. Like it, chlorine is a supporter of combustion, and capable of combining with the metals, and most of the simple combustibles. United to hydrogen, it forms muriatic acid; combined with sulphur and phosphorus, it forms volatile liquids, to which the names of sulphurane and phosphurane have been given by Sir Humphrey Davy. With carbon it does not combine; but it unites with carbonic oxide, and forms the singular acid substance discovered by Mr. Davy, and called by him *phosgene* gas. With some metals it unites in two proportions; with others in one. These combinations are denoted by Sir Humphrey Davy by changing the Latin termination of the metal into *ane* for the first compound, and into *anea* for the second. Thus, the first compound of iron and chlorine is called *ferrane*; the second, *ferranea*.

In this valuable paper Mr. Davy gives an account of many experiments which he made to ascertain the properties and composition of the combinations of metals and chlorine. He has added several experiments on the metallic oxides and sulphurets, to determine the proportion of their constituents with more accuracy than had been hitherto done.

1. Copper combines with two proportions of chlorine. *Cuprane*, the first of these compounds, may be obtained by heating together two parts of corrosive sublimate, and one part of copper filings. In this way it was made by Boyle, and called by him cupreous resin, and compared to benzoin. Proust afterwards described it under the name of *white muriate of copper*. It melts at a heat below redness; in a close vessel is not decomposed nor sublimed by a strong red heat; but in the open air is easily dissipated in dense white fumes. It is insoluble in water, effervesces in nitric acid, dissolves in muriatic acid, from which it is precipitated by water in the state of a white powder.

Cupranea is slowly formed by heating cuprane in chlorine gas. It is easily obtained by evaporating the deliquescent muriate of copper to dryness, at the temperature of 400°. It is a yellow powder, absorbs water from the air, and is converted into deliquescent muriate. It is decomposed by heat, and converted into cuprane even when heated in chlorine gas.

2. Tin, like copper, forms two compounds with chlorine; the liquor of Libavius, one of these compounds, is directly formed by burning the metal in chlorine gas. The other may be obtained by heating together a mixture of amalgam of tin and calomel. Or it may be obtained by evaporating muriate of tin to dryness, and fusing the residue in a close vessel.

Stannane, the last of these compounds, is of a grey colour, and a resinous lustre and fracture. It melts at a heat rather below redness. When heated with corrosive sublimate, nitre, red oxide of mercury, or with hyperoxymuriate of potash, it is changed into liquor of Libavius. It burns in chlorine gas, and undergoes the same change.

The properties of *stannanea*, or liquor of Libavius, have been long known. Mr. Davy found that it acted violently on oil of turpentine, and, in one instance, even set it on fire.

3. Iron likewise combines in two proportions with chlorine. One may be formed directly by burning iron wire in chlorine gas. The other may be formed by heating to redness, in a glass tube, the residue obtained by evaporating green muriate of iron to dryness. It contains the smaller proportion of chlorine, and is therefore ferrane.

Ferrane has a greyish but variegated colour, metallic splendour, and a lamellated structure. It dissolves in water, and forms the green muriate of iron.

Ferranea is a volatile substance, which condenses in the form of small irridescent plates. It dissolves in water, and forms red muriate of iron.

4. A compound of manganese and chlorine may be obtained by evaporating to dryness the white muriate of that metal, and heating the residue to redness in a glass tube with a very small orifice. It is a beautiful substance, of a pink colour, great brilliancy, and a lamellar texture. By forming this compound, and dissolving it repeatedly in water, the manganese may be freed from iron, as the compound of the latter metal with iron is volatile.

5. The compound of chlorine and lead may be obtained by fusing muriate of lead in a glass tube with a narrow orifice. Its properties are sufficiently known.

6. The compound of chlorine and zinc was obtained by evaporating to dryness the muriate of zinc, and heating the residue to redness in a glass tube. Like plumbane, it does not sublime at a red heat. It melts before it is red hot; and on cooling becomes viscid before it acquires hardness. It is very deliquescent.

7. The compound of chlorine and arsenic, or arsenicane, has been long known, under the name of the fuming liquor of arsenic. It may be made by burning arsenic in chlorine gas, or by distilling a mixture of six parts of corrosive sublimate, and one part of arsenic. Its properties are pretty well known. It dissolves sulphur and phosphorus while hot, but lets them fall again as it cools. It dissolves resin, and combines with oil of turpentine and olive oil.

8. Butter of antimony, by which name the combination of antimony and chlorine was formerly known, may be obtained by distilling a mixture of $2\frac{1}{2}$ parts of corrosive sublimate, and one part of antimony. Its properties are well known.

9. Bismuthane may be obtained by distilling a mixture of two parts of corrosive sublimate, and one part of bismuth. It is of a greyish white colour, opake, uncrystallized, and of a granular texture. In a glass tube it bears a red heat without subliming.

The following table exhibits the result of Mr. Davy's experiments on the combinations of different metals with chlorine, oxygen, and sulphur. I have added a column indicating the proportions of these bodies from theory, that it may be seen how nearly Mr. Davy's experiments approach to it.

		By exper.	By theory.		
Copper.... 60	+	32·77	33·6 chlorine make	cuprane.
		67·20	67·2 ditto	cupranea.
		7·79	7·5 oxygen	orange oxide.
		15·00	15·0 ditto	black oxide.
Tin 55	+	33·40	33·6 chlorine	stannane.
	+	67·00	67·2 ditto	stannanea.
	+	15·00	15·0 sulphur	grey sulphuret.
	+	31·00	30·0 ditto	aurum musivum.
	+	7·50	7·5 oxygen	protoxide.
	+	15·20	15·0 ditto	peroxide.
Iron .... 29·5	+	33·60	33·6 chlorine	ferrane.
		55·50	50·4 ditto	ferranea.
		8·00	7·5 oxygen	black oxide.
		13·20	15·0 ditto	red oxide.
Manganese 28·4	+	33·60	33·6 chlorine	manganesane.
Lead.... 97.2	+	33·80	33·6 ditto	plumbane.
	+	15·09	15·0 sulphur	sulphuret.
	+	7·50	7·5 oxygen	yellow oxide.
Zinc.... 34·5	+	34·40	33·6 chlorine	zincane.
	+	7·50	7·5 oxygen	white oxide.
Arsenic.. 21·9	+	33·60	33·6 chlorine	arsenicane.
	+	7·30	7·5 oxygen	white oxide.
Antimony 42·5	+	34·60	33·6 chlorine	antimoniane.
	+	14·86	15·0 sulphur	sulphuret.
	+	7·50	7.5 oxygen	protoxide.
Bismuth 67·5	+	34·20	33·6 chlorine	bismuthane.
	+	15·08	15·0 sulphur	sulphuret.
	+	7·50	7·5 oxygen	oxide.

XI. *Further Experiments and Observations on the Action of Poisons on the animal System.* By B. C. Brodie, Esq. F.R.S.

Mr. Brodie, who has commenced his experimental career with considerable lustre, and promises to throw new light on some of the darkest parts of physiology, published two papers in the Philosophical Transactions for 1811, with the objects of which it is necessary the reader should be acquainted, before he can fully understand the present article. The first of these is entitled, the *Croonian Lecture on some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of Animal Heat.* In this paper Mr. Brodie has shown, by a number of well-conducted experiments, 1. That the influence of the brain is not directly necessary to the action of the heart; for by means of artificial respiration the action of the heart was continued for a considerable time after the head was cut off. Indeed, many phenomena previously known, such as the length of time that the hearts of some animals continue to beat after they are taken out of the body, rendered this conclusion unavoidable. 2. That when the brain is injured or removed, the action of the heart ceases only because respiration is under its influence; and if, under these circumstances, respiration is artificially produced, the circulation will still continue. 3. That when the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated; notwithstanding the functions of respiration, and the circulation of the blood, continue to be performed; and the usual changes in the appearance of the blood are produced in the lungs. 4. That when the air respired is colder than the natural temperature of the animal, the effect of respiration is not to generate but to diminish animal heat. Upon the subject of animal heat I have a few observations to offer; but as Mr. Brodie has another paper on the subject in the Philosophical Transactions for 1812, it will be better to delay them till that paper comes under our review. Mr. Brodie's second paper is entitled, *Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons.* From these very curious experiments Mr. Brodie draws the following conclusions. 1. Alcohol, the essential oil of bitter almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara (a poison used by the Indians in Guiana to poison their arrows) act as poisons by simply destroying the functions of the brain: universal death taking place because respiration is under the influence of the brain, and ceases when its functions are destroyed.

2. The infusion of tobacco when ejected into the intestines, and the upias antiar (a poison used in Java) when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation: in other words, they occasion syncope.

3. There is reason to believe that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves, without being absorbed into the circulation.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided blood-vessels; and from analogy we may conclude, that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison, which acts by simply destroying the functions of the brain, it may, in some instances at least, be made to recover, if respiration be artificially produced and continued for a certain length of time.

In the paper published in the half volume of the Transactions of which we are giving an analysis, Mr. Brodie gives an account of the result of his experiments on the action of some of the most violent mineral poisons upon the animal system. The following are the general conclusions drawn from these experiments.

1. Arsenic, emetic tartar, and muriate of barytes, do not produce their deleterious effects until they have passed into the circulation.

2. All these poisons occasion disorder of the functions of the heart, brain, and alimentary canal; but they do not all affect these organs in the same relative degree.

3. Arsenic operates in the alimentary canal in a greater degree than either tartar emetic or muriate of barytes. The heart is affected more by arsenic than by emetic tartar; and more by this last than by muriate of barytes.

4. Corrosive sublimate, when taken internally in a large quantity, occasions death by acting chemically on the mucous membrane of the stomach, so as to destroy its texture; the organs more immediately necessary to life being affected in consequence of their sympathy with the stomach.

The Second Part of the Philosophical Transactions for 1812 is published; but want of room obliges us to defer our account of it till our next Number.

Article X.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Compound of Chlorine and Azote.*

Some time ago Sir Humphrey Davy received a letter from Paris, containing, among other particulars, a paragraph to the following effect: "I suppose you have heard of the discovery which has been made in France, of a combination of chlorine and azote, a discovery, which cost the person who made it, an eye and a finger." Sir Humphrey Davy, before the receipt of this letter, had made many unsuccessful attempts to produce such a combination. He immediately ran over all the recent French journals, to see whether he might not have overlooked some account of this new compound; but could find no trace of it whatever. While in this perplexity Mr. Children put him in mind of a discovery made some time ago in England, by Mr. Burton, who, by passing a current of chlorine through a solution of nitrate of ammonia, had obtained a white oily looking substance, so volatile, that he had been unable to collect it. On this hint Sir Humphrey Davy immediately set to work, and speedily succeeded in forming the supposed oil. It was volatile; and when heated on the surface of water burnt rapidly, with a feeble puff or explosion. Messrs. Davy, Children, and Warburton, agreed to repair to the laboratory of Mr. Children, and examine this new substance with some care. They succeeded in forming it, by passing a current of chlorine through solutions of oxalate of ammonia, as well as nitrate of ammonia. They put solutions of different ammoniacal salts into a succession of Woulfe's bottles, and passed a current of chlorine through them. The substance in question was formed, and, during the course of the experiment, exploded with prodigious violence, and broke the bottles to atoms. They had recourse in consequence to small tubes, and used quantities of the oil, not exceeding the size of a pinhead. Notwithstanding these precautions, in one of their trials, the tube broke, and a fragment of it entered Sir Humphrey Davy's eye, through the cornea. This untoward accident prevented him from prosecuting the experiments any farther. But they were continued by Messrs. Children and Warburton. The following are the principal facts which they ascertained:

When this substance explodes, a gas is emitted, which when examined turns out to be azotic gas. Hence it seems fair to infer, that the oily substance is a compound of chlorine and azote, and the same substance as that discovered by the French chemist, and alluded to in the letter received by Sir Humphrey Davy, above referred to. When a little of this substance is thrown upon the surface of olive oil or oil of turpentine, a dreadful explosion takes place. It explodes also when it comes in contact with phosphorus, but not with so much violence. During all these explosions considerable heat is evolved.

II. *Sugar from Starch.*

M. Kirchoff, a Russian chemist, has recently discovered a method of converting starch into sugar. Professor Berzelius, who visited London during the last summer, brought the first accounts of this curious process, and some details concerning it have been published by M. Nassé, Member of the Imperial Academy of Petersburgh. The following are the proportions of the different substances used by Kirchoff:

100 parts starch.
400 .... water.
1 .... sulphuric acid.
Powder of charcoal } A sufficient quantity.
Chalk .... }

Half of the water is to be mixed with the sulphuric acid, and brought to a boiling temperature, in a copper vessel well tinned. The starch is to be mixed with the other half of the water. It is then passed through a seirce, and gradually mixed with the boiling acid liquor, in quantities amounting to about half a part at a time. The whole is kept boiling for 36 hours, water being added to supply the place of what evaporates. Then some powdered charcoal is added, and last of all a quantity of chalk, sufficient to saturate the sulphuric acid. After a little additional boiling the liquid is passed through a cloth, to separate the selenite. It is then evaporated by a very gentle heat, to the consistence of a syrup, and set aside for crystallization. The liquid, when it passes through the cloth, ought to be as transparent and colourless as water. The crystallization usually takes place in about three days after the evaporation. No correct details respecting the nature of this sugar have been hitherto published; but it appears to approach nearer to the sugar of grapes than to that which is obtained from the sugar cane.

Attempts have been made to convert wheat flour into sugar by a similar process; but they have not succeeded. The proportion of sulphuric acid may be augmented; but in that case a greater proportion of water must be employed, and the period of boiling must be lengthened; so that in reality nothing is gained by this addition.

This curious process promises to throw additional light on the constitution of vegetable substances. In point of economy very little is gained, starch is in reality dearer than common sugar. Hence, even supposing starch-sugar possessed the requisite qualities, it never could come in competition with common sugar, far less supplant it. At present it would be a much more valuable discovery in this country, to convert sugar into starch, than starch into sugar.

III. *Artificial Grasses.*

Almost the only grass sown in Great Britain is the rye-grass (*Lollium perenne*). This preference is not without reason. It yields the most abundant crop of hay of any of the grasses; and it appears from various trials that have been made, that rye-grass hay is more nourishing than that made from any other species of grass. Another advantage is its earliness, and its leaves are not so coarse as to render it necessary to cut down the grass before it is ripe; so that both hay and seed may be procured from the same field. The *after crop of grass* is likewise luxuriant.

The *alopecurus pratensis*, or *foxtail grass*, has been highly extolled by some farmers, and probably it answers very well for grazing; but it is greatly inferior to rye-grass for the purposes of hay. The *fiorin grass* of Dr. Richardson, which has been considred by every person as the *agrostis stolonifera*, though in reality it is the *agrostis alba*, certainly possesses several excellent qualities, and is probably superior to every other for grazing in marshy soils; but for hay it is entirely unfit. What Dr. Richardson and others have chosen to call hay, being nothing else than the grass still capable of vegetating. It is this capacity that makes it able to endure wet without injury. If it be true that cattle prefer it to hay, we must ascribe the preference to its continuing always in the state of grass. But this unfitness of it for hay renders it less nourishing than the rye-grass; for it is generally known, that a given weight of hay is much more nourishing than the same weight of grass.

The grass usually sown in Sweden is the *phleum pratense*, or *timothy grass*, and the Swedish farmers extol it as much softer and more agreeable to the taste of cattle than rye-grass. How far it possesses these qualities I do not know; but its lateness is an insurmountable objection to its being introduced into this country as an artificial grass; as from it no second crop after the first cutting could be expected. Perhaps in Sweden, where the summer is shorter than in England, this objection may not be of so much consequence; but in England it must be fatal to the introduction of the grass as an object of general culture.

IV. *Egg-Shells.*

Vauquelin has lately examined the constituents of egg-shells, with more care than had been bestowed on them by former chemists, and has found them to consist of the following substances: carbonic acid, lime, magnesia, phosphate of lime, iron, sulphur, and an animal matter which acts as the cement. It is probable, that the lime and the magnesia are combined with carbonic acid. The sulphur seems to be in combination with the animal matter. The state in which the iron exists in egg-shells has not been ascertained. The membrane which lines the internal surface of the egg-shell seems to be coagulated albumen: at least it dissolves in potash without the evolution of any ammonia. Vauquelin detected magnesia, phosphate of lime, and iron, likewise in oyster-shells; but the proportion of these substances was much smaller than in egg-shells. All his attempts to detect uric acid in egg-shells were unsuccessful.

V. *Gunpowder.*

Gunpowder is usually composed of the following constituents:

Nitre	76
Charcoal	15
Sulphur	9

There can be no doubt that the sulphur contributes very materially to the good qualities of the powder, though it is difficult to form an idea of the chemical effect which it produces. Proust has shown, by numerous experiments, that it produces two very important effects. 1. It increases the rapidity of the combustion. 2. It greatly increases the quantity of gaseous matter evolved. Upon these two circumstances the force of gunpowder entirely depends. Proust has shown that the quantity of sulphur ought not to exceed 12·5 grains, supposing the nitre to amount to 75 grains, otherwise the rapidity of the combustion is diminished. Charcoal having a much greater affinity for oxygen than sulphur, the combustion of the gunpowder ought, theoretically speaking, to depend upon the charcoal. There can be little doubt, that it is the charcoal chiefly which burns; though probably the sulphur likewise comes in for a share. It is obvious, that the charcoal can serve no purpose whatever in the powder, except as far as it is consumed. Now 76 parts of nitre contain about 38 parts of nitric acid. The quantity of oxygen in this acid has not been ascertained in an unexceptionable manner; but, by the highest calculation, it cannot amount to more than five times the azote present; and there is reason to believe that it does not fall far short of that quantity. Hence it is probable, that the oxygen present in 76 parts of nitre amounts to 30 parts. We are certain that it cannot exceed that quantity. Now 15 parts of charcoal require for combustion no less than 37·5 parts of oxygen. The quantity of charcoal which would consume 30 parts of oxygen would be 12 parts. From these considerations, it follows, that the best proportions of the constituents of gunpowder would be the following:

Nitre	76
Charcoal	12
Sulphur	12
	100

Or, perhaps, the sulphur might be diminished to ten parts, without impropriety. For it appears from Proust's experiments, that such a proportion of sulphur would produce its full effect. The advantage of these new proportions would be a stronger powder, or at least as strong a powder, two per cent. lighter than the powder at present in use.

VI. *Test of Alumina.*

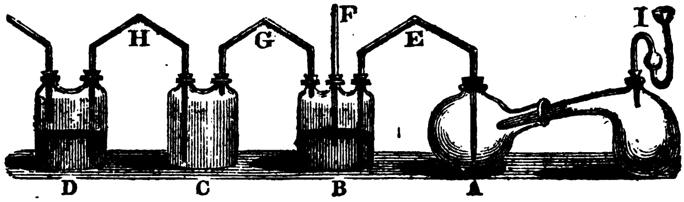
Mr. J. Gotlieb Gahn, the celebrated discoverer of the metallic nature of manganese, and of the composition of the earth of bones, has pointed out a valuable test for discovering the presence of alumina in mineral substances exposed to the action of the blow-pipe. It is as follows. Place upon the substance to be tried a drop of nitrate of cobalt, and expose it to the white flame of the blow-pipe. If the mineral contain alumina in any quantity, and is not too much charged with iron, or other colouring metals, it soon acquires a blue colour, more or less brilliant, and more or less intense; according to the purity and the abundance of the alumina which it contains. "I had never," says M. Gahn, in a letter to the editor of this journal," extended the application of this experiment to minerals of a very great degree of hardness, such as the sapphire, the spinell, corundum, topaze, &c.; but the other day I was under a kind of necessity of making the trial, and I observed, with pleasure, that all these substances exhibited the blue colour as well as others. It is only necessary to pulverize them well, and expose them to the action of the blow-pipe in the manner above described. This test labours under one disadvantage, however; for the earth of zircon produces the same blue colour with cobalt as alumina does."

VII. *Extracts from two Letters of Dr. John Redman Coxe, Professor of Chemistry in Philadelphia.*

1. "It has long been unknown upon what principle the combustion of pyrophyrus depended. Mr. Davy, since his discovery of potassium, has ascribed it to its presence in some way. A few weeks past I had occasion to lecture on this substance (pyrophyrus); and finding a portion, which I had prepared some time before (but which had failed, except in one or two instances), to be useless, I was under the necessity of making some fresh. After the first portions of gas had escaped, and the inflammable gas began to come over, I was, near the period I intended to stop the process, struck with the appearance of the flame, which possessed, as I thought, the rose-coloured flame of potassium. I immediately stopt the process; and obtained as fine pyrophyrus as I ever met with. Reasoning upon the subject, I was led to suppose that the frequent failure of the process depended on our carrying it so far, that the potassium formed was entirely consumed. In consequence of this, I poured out all my *old* pyrophyrus, and found that no combustion ensued. To this mass I added 30 or 40 drops of a pure alkaline solution (potash), and exposed it, as usual, to heat, in a crucible surrounded with sand. Inflammable gas began to escape, and at length appeared tinged with a rose-coloured flame; when I stopt the process, and when cool poured the contents into a dry warm vial. This possessed, in a very excellent degree, the properties of perfect pyrophyrus. I apprehend, in this compound, the potassium is diffused through the mass in its metallic state, and, seizing on the atmospheric moisture, gives origin to some potassuretted hydrogen, which, inflaming by the contact of oxygen, communicates combustion to the carbonaceous materials surrounding it. If it existed in any other form than metallic, ought it to be so long, as we sometimes see it, before

combustion ensues? and why should moisture be necessary often, as by breathing on it? In this preparation, either not carrying on the heat sufficiently long to metallize the potash, or, beyond this, to its complete burning off, will equally prejudice it. If my idea was correct, as to the colour of the flame, we may have a criterion to judge when to stop."

2. "In places where Welter's tubes of safety cannot be had, such an arrangement of Woulfe's apparatus as will enable an operator to saturate water with the gases, without danger from absorption, cannot fail of being acceptable. The following plan explains such a method, which supersedes the use of Welter's tubes, and will permit the absence of the operator, without danger from absorption.



"The tube E is twice bent at right angles, with legs of equal length; one descends to the bottom of the tubulated receiver, A; the other to the bottom of a three-necked bottle, B. In this bottle is placed the only tube of safety, F, which is necessary, descending about half an inch below the surface of the water.

"The tube G is twice bent, and only perforates the corks, without descending into the bottles B and C.

"The tube H, bent as before, descends to the bottom of the bottles C and D. Other bottles may be added at pleasure, connected as C and D; that is, having the tubes near the bottom of each bottle; or, perhaps, rather as represented above.

"If the apparatus is wanted to saturate water with muriatic acid gas, a little water must be put in B, as usual, to absorb any sulphurous acid gas evolved; C is left empty; and into D, &c. must be introduced the distilled water to be saturated.

"If during the absence, or from inattention of the operator, an absorption should take place, the water in B will pass up the tube E, and be deposited on the bottom of the empty receiver. When the orifice of the tube F is exposed, the air will rush into the bottle B; and by its presence, if the absorption is great, will force the remaining water into the receiver, until the orifice of the tube E is exposed, when the air will pass through the tube E, and, rising through the water in the receiver, restore the equilibrium.

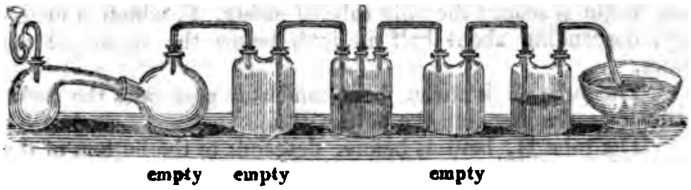
"A part of the water in bottle D will rise simultaneously with the solution in B, and pass into C; but as the tube G does not descend to the bottom, it can pass no farther, and is preserved from contamination by the sulphurous gas in the bottle B. Further, after the orifice of tube F is exposed, the atmospheric air, rushing in through G, arrests the further passage of the liquid of D.

"When an absorption has thus taken place, by exciting a fresh action in the retort, the gas accumulates in the receiver; and when sufficient to support a column of fluid of the height of one leg of the tube E, the fluid in the receiver will be forced into the bottle B again; when the gas will follow and pass into C; when, acting on the surface of the liquid that may have come over into it, it forces it back through H into D, and the process of saturation will proceed as before.

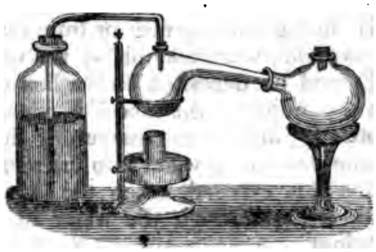
"If a bent funnel is used, as I, the equilibrium is still sooner restored, by the air passing through it at once into the retort.

"In obtaining nitric acid, when the strong acid is desired to be kept unmixed in the receiver, it will be advisable to place an empty bottle between the receiver and bottle B; connecting it and the receiver as B and C; and connecting B with it, as B is above connected with the receiver.

"Since writing the above I have tried the apparatus, I think very fairly; and find it answer all I could wish. Where, however, the extrication of gas is very sudden, and large in amount, it will be best to leave each alternate bottle empty. Thus:



"The Gentleman who has improved this apparatus, as described above, has, within a few days, suggested the plan of a self-registering contrivance to send over the acid which may be required in the retort for the decomposition of the materials. This I mean to try in a few days. The plan is simply thus: From the tubular of the retort goes a bent tube into a bottle of (say) sulphuric acid to the bottom. When absorption of the extricated gas, from the materials in the retort, takes place, the atmospheric pressure will force some acid up the



&c.

tube, and it will pass into the retort; and this will occur as often as the acid is so far neutralised as to be unable to keep up an adequate action. You will comprehend the intention by this rough sketch—for which I beg you to pardon me, as I am unexpectedly engaged, and must finish this for the packet about to sail."

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The Royal Society, as usual, resumed its sittings on Thursday the 5th of November. The Right Hon. Sir Joseph Banks, Bart. in the chair. A letter from Sir Humphrey Davy to the President was read, giving some account of a new and very extraordinary detonating compound. This is the combination of chlorine and azote, of which we have given some account in the preceding article. Our information on the subject was chiefly derived from Sir Humphrey Davy's letter. We shall only add here the method of preparing the compound. Expose a weak solution of nitrate of ammonia to chloric gas in a jar; the gas is absorbed, and after a certain time a yellow oily-looking substance is seen floating on the surface of the solution. This is the substance in question. Great caution is requisite in making experiments on it. Oxalate of ammonia, and several other ammoniacal salts, were tried instead of nitrate, and were found to answer the purpose.

On the 12th of November, a paper by the Astronomer Royal, Mr. Pond, was read, on the summer solstice and the mural quadrant at Greenwich.

On the 19th, a paper on near sight, and the best remedies for defective vision, by Mr. Ware, was read. There is reason to believe, from the observations of Mr. Ware, that this disease is much promoted by the use of glasses; and that if glasses are not employed, it soon wears off and disappears. Hence it is much more common among the higher ranks than among the common people. In the regiments of Life Guards Mr. Ware did not find a single person afflicted with the disease, and not above five or six recruits had been dismissed on account of defective vision; while in one of the colleges at Oxford, consisting of 125 students, no less than 37 were near sighted.

On the 26th, the Bakerian lecture was read by Dr. William Hyde Wollaston, on the constitution of those crystalline bodies whose primitive form is the octahedron or tetrahedron. It is well known to crystallographers, that a kind of anomaly exists with respect to these bodies, as far as regards their primitive form. When an octahedron is subjected to mechanical division, it is separated into octahedrons and tetrahedrons. The same holds with regard to the tetrahedron. If we conceive the octahedron to be the primitive form of such crystals, it is obvious that a congeries of octahedral particles, when connected together, will leave between them tetrahedral cavities. If the tetrahedron be chosen as the primitive form, we must then suppose the crystal full of octahedral cavities. Hence a difficulty arises. Haüy has made choice of the tetrahedron as the primitive form. Dr. Wollaston, with his usual ingenuity, has shown, that if we suppose the atoms of such crystals to be spheres, such bodies could arrange themselves into tetrahedrons or octahedrons, without the necessity of supposing such vacuities. Dr. Wollaston applied this ingenious idea, which he mentioned as originally suggested by Dr. Hooke, to the constitution of bodies in general, and showed how all crystalline figures could be composed of particles having the shape of spheres, or sometimes of flat spheroids.

On Nov. 30, or St. Andrew's day, the Society met, as usual, to elect their office-bearers for the ensuing year: when the following Noblemen and Gentlemen were elected :—

PRESIDENT, Right Hon. Sir Joseph Banks, Bart.
SECRETARIES, Dr. William Hyde Wollaston.
Taylor Combe, Esq.
TREASURER, Samuel Lysons, Esq.
FOREIGN SECRETARY, Dr. Thomas Young.

OF THE OLD COUNCIL.

Right Hon. Sir Joseph Banks, Bart. K. B. Pres.
Sir Charles Blagden.
Samuel Goodenough, Lord Bishop of Carlisle.
Anthony Carlisle, Esq.
Sir Humphrey Davy.
Samuel Lysons, Esq. Treasurer.
Joseph de Mendoza Rios, Esq.
George Earl of Morton.
John Pond, Esq. Astronomer Royal.
Dr. William Hyde Wollaston, Sec.
Dr. Thomas Young, Foreign Sec.

OF THE NEW COUNCIL.

George Earl of Aberdeen.
Taylor Combe, Esq. Sec.
Sir Thomas Frankland, Bart.
Sylvester Lord Glenbervie.
Philip Earl of Hardwicke.
Matthew Raper, Esq.
Samuel Rogers, Esq.
Smithson Tennant, Esq.
Rev. William Tooke.
Roger Wilbraham, Esq.

At the time of the election, the Society consisted of 608 Members; namely, 565 Ordinary Fellows, and 43 Foreign Members.

Dec. the 10th, a paper on ulmin, by Mr. Smithson, was read. Mr. Smithson was induced to make his experiments, in consequence of the account of ulmin published in Dr. Thomson's System of Chemistry. He had received a portion of the same ulmin from Palermo, upon which Klaproth had experimented; and it was upon this portion that his experiments were made. This circumstance renders Mr. Smithson's observations peculiarly valuable. They differ, indeed, but little from the account of ulmin given in an article contained in this very number of our journal, which was printed before Mr. Smithson's paper was read. But as the experiments of Dr. Thomson were made upon English ulmin, it might have been objected that the reason why they did not correspond with those of Klaproth was, that the substance which he examined was of a different nature. The experiments of Mr. Smithson quite destroy this objection. He observed, that the solution of ulmin in water reddened vegetable blues. This is a very remarkable circumstance, if we consider the quantity of potash which ulmin contains, and can only be accounted for by supposing the potash saturated with an acid. The most probable acid is the acetic. The ulmin examined in Article III of this number did not redden vegetable blues; so that in this respect the two specimens differ materially from each other. They seem to differ also in the proportion of potash: the English ulmin containing more than the specimen from Palermo. Mr. Smithson considers ulmin as approaching very nearly to extractive in its properties. There can be no doubt of the truth of the remark. Extractive, like ulmin, is precipitated from its solution in water by acids, and by several metalline salts. But there is one difference between extractive and ulmin too striking to admit them to be classed together. Extractive dissolves in alcohol, but ulmin is insoluble in that liquid. It is doubtless true, that species of extractive can be named that refuse to dissolve in alcohol. But this only shows that at present very dissimilar bodies are classed together improperly, under the name of extractive, and that the class should be divided into at least two new genera; and we may distinguish these genera by the names of *extractive* and *ulmin*.

Dec. 17. A paper on freezing at a distance, by Dr. Wollaston, was read. The author began by mentioning the well-known fact respecting the latent heat necessary to convert water into ice, (140 degrees), and that which converts water into steam (960 degrees), and explained by that means the lowering of the temperature of water, which takes place by evaporation. To freeze water by the air-pump, a very complete vacuum is requisite, and likewise a large receiver. Mr. Leslie's ingenious method of exposing the vapour raised to a surface of sulphuric acid diminished the size requisite, and saved a great deal of labour. Dr. Wollaston's apparatus is still more simple. It consists of a glass tube of some length, having two balls of an inch in diameter, one at each extremity. One of the balls is to be nearly half full of water, the other empty; and both balls and tube must be as completely as possible exhausted of air. The tube is to be bent into a right angle at a little distance from each tube. If the empty ball be plunged into a mixture of salt and snow, the water in the other ball very speedily freezes, though at a distance, in consequence of the rapid condensation of the vapours by the cold. This little instrument Dr. Wollaston calls a chryophorus.

A catalogue of the position of some of the stars near the pole, ascertained by means of the new circular instrument at Greenwich, by the Astronomer Royal, Mr. Pond, was presented to the Society. By this instrument, he states, the position of stars may be determined with a degree of accuracy before unknown in astronomy.

A paper by Sir Everard Home, Bart. was likewise read, on the organs of digestion of several birds. These organs were particularly described; but we need not attempt an account of it, as it would not be intelligible without figures. The paper concluded with a comparison between the organs of digestion of birds of the same kind, showing how admirably they are adapted to the situation in which the animal is placed. The cassuary of Java, one of the most fertile islands in the world, where food is abundant, has small digestive glands, a small gizzard, and an enormous length of entrails. In the cassuary of New Holland, where the food is more scanty, the glands and gizzard are larger, and the entrails shorter. In an animal of the same kind in the deserts of Africa, where food is still more scarce, the glands and gizzard are still larger, and the entrails shorter.

A paper was also read by Mr. Brande, confirming, by farther experiments, the conclusions which he had formerly drawn, respecting the quantity of alcohol in wines of different kinds. It had been objected to his former conclusions, that the alcohol which he had obtained might have been formed during the process of distilling the wine, by which means he obtained it. In this paper he describes the method which he fell upon to separate alcohol from wine without the application of any heat. When subacetate of lead is dropt into port wine, a copious precipitate falls, consisting of the whole colouring matter, and the tartaric acid of the wine, in combination with the oxide of lead. When the liquid is now filtered it is colourless, and if it be mixed with a sufficient quantity of dry submuriate of potash, the alcohol speedily separates, and floats upon the surface of the liquid. By this process he obtained very nearly as much alcohol as he did by his former process of distillation. From farther experiments Mr. Brande conceives, that the quantity of alcohol formerly stated as existing in port wine is rather higher than the truth. He now rates it at 22 per cent. by measure. One specimen of port wine examined yielded only 19 per cent.; but it was very weak wine. The author considers these experiments as fully warranting his preceding conclusions, respecting the quality of alcohol existing in wine.

Dec. 24. A paper on arsenic, by Dr. Lambe, was read. Dr. Lambe distilled a mixture of quick-lime, iron-filings, and white oxide of arsenic, in a retort. He obtained a considerable quantity of carbonic acid gas, and a gas which he calls nitrous carbonic oxide. It was inflammable, and, according to him, contained azote, carbon, and oxygen. He obtained likewise abundance of carbonic acid gas, when a mixture of quick-lime (which did not effervesce with muriatic acid) and white arsenic was distilled. We regret that Dr. Lambe did not in these experiments ascertain the purity of the white oxide of arsenic which he employed. The want of attention to this circumstance leaves us at a loss to account for the production of carbonic acid gas which he obtained.

LINNÆAN SOCIETY.

The last three meetings of the Linnæan Society were chiefly employed in hearing read a monography of the genus *callitriche*, by Mr. Schmaltz.

The paper was in English, of considerable length, and gave a very minute account of this genus, together with an elaborate historical detail of the labours of former botanists, and an ample account of every thing relating to the mode of growth, economy, and uses of this genus of plants, which, it is well known, grow in water, frequently cover the surface of lakes, and are sometimes so matted together, that, if we believe Linnæus, it is possible to walk over the surface of the water without being wetted. According to Mr. Schmaltz there are six species of callitriche peculiar to the old continent; eight belong to the new; and there are two species found both in the old and new continents.

A paper by Mr. Sowerby was likewise read, giving an account of a new cleavage, which he had observed in calcareous spar; namely, a cleavage corresponding to the diagonal of the rhomboid, which constitutes the primitive form of calcareous spar. Mr. Sowerby probably was not aware, that this cleavage had been already noticed and described by preceding writers on the subject. If we remember right, for it is not in our power at present to consult the original paper, it is noticed by Mohs, in a paper in which he describes no fewer than 14 different cleavages which he had observed in calcareous spar.

Article XII.

New Patents.

1. Louis Honoré Henry Germain Constant, of Blandford-street, Portman-square, in the parish of St. Mary-le-bone, and county of Middlesex, for a method of refining sugars. Dated February 27, 1812.

Mr. Constant's method consists in making a very pure syrup, and making it pass slowly through the raw or loaf sugar to be refined. According to him, it drives before it the molasses, or coloured syrup, and takes its place. The pure syrup is made by dissolving raw sugar in water, heating the solution, mixing it with five pounds of finely-pounded charcoal for every hundred weight of sugar, adding the usual proportion of blood, bringing the syrup to boil, stopping the heat by means of a metal plate drawn under the boiler, and then skimming off the albumen and charcoal which collects on the surface. We have only to observe, that this process of Mr. Constant's is not quite new. Something very similar was proposed by Sir Humphrey Davy, in one of his lectures, several years ago: and about a year ago Mr. Edward Howard suggested a similar method in the evening paper called the Star, and likewise in the Philosophical Magazine.

2. Frederick Albert Winsor, of Shooter's-hill, in the county of Kent, Esq. for a method of employing raw and refined sugars in the composition of sundry articles of merchandize in great demand, where it has not heretofore been used. Dated December 4, 1811.

Mr. Winsor's proposal is to mix together three parts of gunpowder, and one part of well triturated sugar. The mixture, he says, will have the force of four parts of gunpowder. We are very much disposed to doubt this conclusion. It is true, as Mr. Winsor says, that sugar is a very combustible substance: but it is not capable, like gunpowder, of supporting its own combustion. If Mr. Winsor chuses to try the experiment of substituting sugar for gunpowder in a fowling-piece, he will find it quite inefficacious. Yet it contains as much oxygen as the same weight of saltpetre does. The fault of gunpowder is, that it contains too much combustible matter already. It would be made stronger by diminishing the proportion of charcoal which it contains. We have no doubt that the addition of sugar will, in reality, diminish the force of gunpowder, by making it burn more slowly than it otherwise would.

Sugar, indeed, would constitute an useful article in fire-works. But we have no doubt, from the appearances which we have seen in fire-works, that sugar has been used in them from time immemorial.

3. William Hardcastle, of Abingdon, in the county of Berks, Gent. for an improvement in cranes, to prevent accidents from the rapid descent of heavy bodies. Dated May 26, 1812.

This improvement consists in making the rope that supports the weight pass round two cylinders, which, by counteracting each other, prevent the weight from rolling down spontaneously.

4. George Smart, of Ordnance Wharf, in the city of Westminster, timber merchant; for a method of preparing timber, whereby the same is prevented from shrinking. Dated May 5, 1812.

This method consists in compressing the wood, by a force sufficient to reduce it to dimensions as small as those into which it ever can shrink.

5. Col. William Congreve, of Cecil-street, in the city of Westminster, and county of Middlesex; for an improved system of securing buildings, towns, dock-yards, and ships, from fire; combining a power for the raising of water to the tops of buildings, and for other general purposes. Dated October 31, 1812.

6. Edward Charles Howard, of Westbourne Green, in the county of Middlesex; for a process for preparing and refining sugars. Dated October 31, 1812.

7. William Caslon, the younger, of Dorset-street, Salisbury-square, in the city of London, letter founder; for an improved printing type. Dated October 31, 1812.

8. Joseph Bramah, of Pimlico, in the county of Middlesex, engineer; for certain improvements in the method of constructing, laying down, and organizing, the main and other pipes for the conveyance of water, for the supply of the metropolis of London, and other cities, towns, and places, where public water-works are adopted, and applying the water so conveyed to a variety of other useful purposes. Dated October 31, 1812.

9. William Evetts Sheffield, of Somer's-town, in the county of Middlesex, Gent.; for an apparatus and furnaces for separating metallic and other substances from their ores, or whatever matters may be combined, united, or mixed with them, and in the application of the same. Dated October 31, 1812.

10. Thomas Lea, of Kidderminster, in the county of Worcester, carpet manufacturer; for certain improvements in the making of carpets. Dated October 31, 1812.

11. Edward Jukes, of Walworth, in the county of Surrey, Gent.; for an instrument, or shears, for pruning of trees, gathering grapes and other fruits, and for cutting off such limbs as may be injured, and thereby more easily destroy the insects occasioned by blights, which instrument he denominates the *Averruncator*. Dated November 7, 1812.

Article XIII.

Scientific Books in hand, or in the Press.

Sir Humphrey Davy will shortly publish Elements of Agricultural Chemistry, in a Course of Lectures delivered before the Board of Agriculture.

The Second Part of Mr. Playfair's Outlines of Natural Philosophy is announced as in the Press; also a New Edition with Additions of his Illustrations of the Huttonian Theory.

Mr. Accum is preparing Elements of Crystallography, with or without a Series of Geometrical Models, both solid and dissected, exhibiting the Forms of Crystals, their Geometrical Structure, Dissections, and General Laws, according to which that immense Variety of actual existing Crystals are produced.

Mr. Keith will shortly publish, in one volume, the Elements of Plane Geometry, comprehending the First Six Books of Euclid, from the Text of Dr. Simson, with Notes, Critical and Explanatory. To which is added, Book VII. containing several important Propositions which are not in Euclid, and Book VIII. consisting of Practical Geometry.

A New Edition of Mr. Smeaton's Eddystone Lighthouse is in forwardness.

A Translation of Corvisart's Work on the Diseases of the Heart and Great Vessels, has been undertaken by Mr. Hebb, of Worcester.

Dr. Pritchard, of Bristol, is preparing a Volume of Researches into the History of the Human Kind, and the Nature of Physical Diversities.

Mr. Lovell, Building Surveyor, Huntingdon, has in the Press, The Builder's Assistant, comprising a New System of Duodecimal Arithmetic, with a variety of newly-constructed Tables for finding the Amount of any number of Feet and Inches, Yards and Feet, Squares and Feet, and Rods and Feet, at any given price.

A New Edition of Dr. Hutton's Mathematical and Philosophical Dictionary is prepared for the Press: the Additions are very numerous, and the Work is brought down to the present time.

Mr. Charles Bell is preparing for publication, Engravings from Specimens of Morbid Parts preserved in his Collection, and selected from the Divisions inscribed Urethra, Vesica, Ren, Morbosa et Læsa.

*** *Early Communications for this Department of our Journal will be thankfully received.*

Article XIV.

METEOROLOGICAL JOURNAL.

1812.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap	Rain.	
10th Mo.										
Oct. 27	S W	29·40	29·24	29·320	52	40	46·0	—	·26	
28	S W	29·75	29·40	29·575	51	32	41·5	—		
29	W	29·80	29·74	29·770	49	33	41·0	·15		
30	S E	29·78	29·66	29·720	51	39	45·0	—	·14	
31	W	29·94	29·78	29·860	54	41	47·5	—		☽
11th Mo.										
Nov. 1	S W	29·94	29·87	29·905	55	50	52·5	—	—	
2	N	30·05	29·87	29·960	54	44	49·0	7	·65	
3	S W	—	—	—	49	38	43·5	—		
4	W	30·05	29·83	29·940	48	39	43·5	8		
5	N W	29·83	29·80	29·815	49	30	39·5	—		
6	S W	29·83	29·80	29·815	45	27	36·0	—		
7	W	29·83	29·79	29·810	45	24	34·5	—		
8	E	29·79	29·77	29·780	40	27	33·5	5		●
9	E	—	—	—	46	39	42·5	—		
10	N E	30·15	30·03	30·090	45	33	39·0	—		
11	S E	—	—	—	46	39	42·5	—	·32	
12	E	30·03	29·58	29·805	45	39	42·0	—	·38	
13	N E	29·58	29·20	29·390	51	44	47·5	—	·55	
14	W	29·59	29·20	29·395	52	40	46·0	—	—	
15	S W	29·59	29·30	29·445	52	39	45·5	—	3	
16	N E	29·30	29·00	29·150	46	42	44·0	—	—	☽
17	N E	29·00	28·96	28·980	46	40	43·0	—	—	
18	N W	29·66	28·96	29·310	42	32	37·0	7	·13	
19	N	29·86	29·66	29·760	42	28	35·0	—		
20	N	29·97	29·83	29·900	41	33	37·0	—	—	
21	N E	30·32	29·97	30·145	39	26	32·5	7		
22	N	30·38	30·31	30·345	43	25	34·0	—		○
23	S W	30·31	30·08	30·195	44	26	35·0	—		
24	S W	30·08	29·89	29·985	48	39	43·5	9		
		30·38	28·96	29·678	55	24	41·31	0·58	2·46	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Tenth Month. 27. Misty and overcast, a. m. Wet at noon: p. m. the barometer descended at the rate of a tenth of an inch per hour, the wind increasing in proportion, with much rain, the clouds sweeping the earth. The evening was very tempestuous: before midnight the barometer had risen again, and the weather became moderate. Many large trees were blown down. 28. a. m. Hoar frost, rather misty. At sunset, the sky exhibited a fine collection of coloured clouds, in the modifications *Nimbus* and *Cirrus*, together with broad parallel bands of red, in the haze above them. 29. Fair and calm. 30. *Cirrostratus* and *Cumulus*; the sky again beautifully coloured.

Eleventh Month. 1. Cloudy. 2. a. m. Wet. 5. Fine day. 6, 8, 9, 10, 11. Chiefly misty or cloudy, with hoar frost, and some very thick local fogs. 11. Overcast, a. m. The *Cirrostratus* prevails, and sounds travel with the wind to an unusual distance: we hear the rattling of the carriages on the pavement in London, through a direct mean distance of five miles. This phenomenon is to be attributed to a thick continuous sheet of haze in the air above us, which acts as a sounding board. 12. Rain through the day. 13. Misty; rain: sounds are again distinctly heard from the city. 15. Fair: a *Stratus* at night. 16. Overcast; with an easterly gale. 18. Wet and stormy day: night clear and calm. 20. Misty: rime on the trees, which came off, about noon, in showers of ice. At 11 a. m. a perfect, but colourless *bow*, in the *mist:* near 4 p. m. there was a shower, in which the rainbow showed it's proper colours. 22. Clear: the ground just sprinkled with hail balls. 23. a. m. Misty; rime: p. m. clearer: thaw in the night. 24. Clear morning.

RESULTS.

Winds (for the greater part) westerly: the easterly have been most associated with rain.

Barometer: highest observation 30·38 inches; lowest 28·96 inches; Mean of the period 29·678 inches.

Thermometer: greatest height 55°; least 24°; Mean 41·31°.

Evaporation 0·58 inches. Rain 2·46 inches.

PLAISTOW, L. HOWARD.
Twelfth Month, 18, 1812.

NOTE BY THE EDITOR.

Luke Howard, Esq. to whom we are indebted for our Meteorological Table, some years ago contrived a nomenclature for the different appearances of the clouds. As this nomenclature is employed in the preceding observations, we think it right to give a short explanation of it here. The terms are seven.

1. *Cirrus.* Parallel, flexuous, or diverging fibres, extensible in any or in all directions.

2. *Cumulus.* Convex or conical heaps, increasing upwards from a horizontal base.

3. *Stratus.* A widely extended, continuous, horizontal sheet, increasing from below.

4. *Cirro-cumulus.* Small, well defined, roundish masses, in close horizontal arrangement.

5. *Cirro-stratus.* Horizontal or slightly inclined masses, attenuated towards a part or the whole of their circumference, bent downwards or undulated, separate or in groups, consisting of small clouds having these characters.

6. *Cumulo-stratus.* The cirro-stratus blended with the cumulus, and either appearing intermixed with the heaps of the latter, or superadding a wide-spread structure to its base.

7. *Cumulo-cirro-stratus,* or *Nimbus.* The rain cloud. A horizontal sheet above, which the cirrus spreads, while the cumulus enters it laterally and from beneath.

It will be observed that Mr. Howard begins his journal at the moon's last quarter.

ANNALS

OF

PHILOSOPHY.

FEBRUARY, 1813.

Article I.

Biographical Account of Joseph Priestley, LL.D. F.R.S. By Thomas Thomson, M.D. F.R.S.

Dr. Joseph Priestley was born in 1733, at Fieldhead, about six miles from Leeds, in Yorkshire. His father, Jonas Priestley, was a maker and dresser of woollen cloth; and his mother, the only child of Joseph Swift, a farmer in the neighbourhood. Dr. Priestley was the eldest child; and, his mother having children very fast, he was soon committed to the care of his maternal grandfather. He lost his mother in 1739, when only six years of age; and was soon after taken home by his father, and sent to school in the neighbourhood. His father being but in poor circumstances, and encumbered with a large family, his sister, Mrs. Keighly, a woman in good circumstances, and without children, relieved him of all care of his eldest son, by taking him, and bringing him up as her own. She was a Dissenter; and her house was the resort of all the dissenting clergy in the country. Young Joseph was sent to a public school in the neighbourhood, and at sixteen had made considerable progress in Latin, Greek, and Hebrew. Having shown a passion for books and for learning at an early age, his aunt conceived hopes that he would one day become a clergyman, which she considered as the first of all professions; and he entered very eagerly into her views: but his health declining about this period, and being threatened with a consumption, he was advised to turn his thoughts to trade, and to settle as a merchant in Lisbon. This induced him to apply to the modern languages; and he learned French, Italian, and German, without a master. Recovering his health, he abandoned this new scheme, and resumed his former plan of becoming a clergyman. In 1752 he was sent to the academy of Daventry, to study under Mr. Ashworth, the successor of Dr. Doddridge. He had already made some progress in mechanical philosophy and metaphysics, and acquired some knowledge of Chaldee, Syriac, and Arabic. At Daventry he spent three years, engaged keenly in studies connected with divinity, and wrote some of his earliest theological tracts. Freedom of discussion was admitted in its full extent in this academy. The two masters espoused different sides upon most controversial subjects, and the scholars were divided into two parties, nearly equally balanced. The discussions, however, were carried on with perfect good humour on both sides; and Dr. Priestley, as he tells us himself, usually supported the heterodox opinion: but he never at any time, as he assures us, advanced arguments which he did not believe to be good, or supported an opinion which he did not consider as true. When he left the academy he settled at Needham, in Suffolk, as assistant in a small obscure dissenting meeting-house, where his income never exceeded 30l. a-year. His hearers fell off, in consequence of their dislike to his theological opinions; and his income underwent a corresponding diminution. He attempted a school; but his scheme failed of success, owing to the bad opinion which his neighbours entertained of his orthodoxy. His situation would have been desperate, had he not been occasionally relieved by sums out of charitable funds procured by means of Dr. Benson and Dr. Kippis.

Several vacancies occurred in the neighbourhood; but he was treated with contempt, and thought unworthy to fill any of them. Even the dissenting clergy in the neighbourhood considered it as a degradation to associate with him, and durst not ask him to preach: not from any dislike to his theological opinions, for several of them thought as freely as he did, but because the genteeler part of their audience always absented themselves when he appeared in the pulpit. A good many years afterwards, as he informs us himself, when his reputation was very high, he preached in the same place, and multitudes flocked to hear the very same sermons which they had formerly listened to with contempt and dislike.

His friends, being aware of the disagreeable nature of his situation at Needham, were upon the alert to procure him a better. In 1758, in consequence of the interest of Mr. Gill, he was invited to appear as a candidate for a meeting-house in Sheffield, vacant by the resignation of Mr. Wadsworth. He appeared accordingly, and preached, but was not approved of. Mr. Haynes, the other Minister, offered to procure him a meeting-house at Nantwich, in Cheshire. This situation he accepted; and, to save expence, went from Needham to London by sea. Here he continued three years, and spent his time much more agreeably than he had done at Needham. His opinions were not obnoxious to his hearers, and controversial discussions were never introduced. Here he established a school, and found the business of teaching, contrary to his expectation, an agreeable and even interesting employment. He taught from seven in the morning till four in the afternoon; and after the school was dismissed he went to the house of Mr. Tomlinson, an eminent attorney in the neighbourhood, where he taught privately till seven in the evening. Being thus engaged in teaching twelve hours every day, he had but little leisure for his private studies. Here, however, his circumstances began to mend. At Needham it required the utmost economy to keep out of debt; but at Nantwich he was able to purchase a few books, and some philosophical instruments, as a small air-pump, an electrical machine, &c. These he taught his oldest scholars to keep in order, and to manage; and by entertaining their parents and friends with experiments, in which the scholars were generally the operators, and sometimes the lecturers too, he considerably extended the reputation of his school. It was at Nantwich that he wrote his grammar for the use of his scholars; a book of considerable merit, though its circulation was never extensive. This, I presume, was owing to the superior reputation of Dr. Lowth, who published his well-known grammar about two years afterwards.

Being boarded in the house of Mr. Eddowes, a very sociable and sensible man, and a lover of music, Dr. Priestley was induced to learn to play a little on the English flute; and though he never was a proficient, he informs us that it contributed more or less to his amusement for many years. He recommends the knowledge and practice of music to all studious persons; and thinks it rather an advantage for them if they have no fine ear or exquisite taste, as they will in consequence be more easily pleased, and less apt to be offended when the performances they hear are but indifferent.

The academy at Warrington was instituted while Dr. Priestley was at Needham, and he was recommended by Mr. Clark, Dr. Benson, and Dr. Taylor, as tutor in the languages; but Dr. Aikin, whose qualifications were considered as superior, was preferred before him. However, on the death of Dr. Taylor, and the advancement of Dr. Aikin to be tutor in divinity, he was invited to succeed him. This offer he accepted, though his school at Nantwich was likely to be more gainful: but the employment at Warrington was more liberal and less painful. In this situation he continued six years actively employed in teaching and in literary pursuits. Here he wrote a variety of works, particularly his *History of Electricity*, which first brought him into notice as an experimental philosopher, and procured him celebrity. After the publication of this work, Dr. Percival, of Manchester, then a student at Edinburgh, procured him the title of Doctor of Laws from that University. Here he married a daughter of Mr. Isaac Wilkinson, an ironmonger in Wales; a woman whose good qualities he has highly extolled, and who died after he went to America.

In the academy he spent his time very happily, but it did not flourish. A quarrel had broken out between Dr. Taylor and the trustees, in consequence of which all the friends of that gentleman were hostile to the institution. This, together with the smallness of his income, 100l. a-year, and 15l. for each boarder, which precluded him from making any provision for his family, induced him to accept an invitation to take charge of Mill-hill chapel, at Leeds, where he had a considerable acquaintance, and to which he removed in 1767.

Here he engaged keenly in the study of theology, and produced a great number of works, many of them controversial. Here, too, he commenced his great chemical career, and published his first tract on *Air*. He was led accidentally to think of pneumatic chemistry, by living in the immediate vicinity of a brewery. Here, too, he published his history of the *Discoveries relative to Light and Colours*, as the first part of a general history of experimental philosophy: but the expense of this book was so great, and the sale so limited, that he did not venture to prosecute the undertaking. Here, likewise, he commenced and published three volumes of a periodical work, entitled the *Theological Repository*, which he continued after he settled in Birmingham.

After he had been six years at Leeds, the Earl of Shelburne (afterwards Marquis of Lansdowne) engaged him, at the recommendation of Dr. Price, to live with him as a kind of librarian and literary companion, at a salary of 250l. a-year and a house. With his Lordship he travelled through Holland, France, and part of Germany, and spent some time in Paris. He was delighted with this excursion, and expressed himself as thoroughly convinced of the great advantage to be derived from foreign travel. All the philosophers and politicians in Paris were unbelievers, and even professed Atheists; and as Dr. Priestley chose to appear before them as a Christian, they told him he was the first person they had met with of whose understanding they had any opinion who was a believer of Christianity: but upon interrogating them closely, he found that none of them had any knowledge either of the nature or principles of the Christian religion. While with Lord Shelburne, he published the first three volumes of his *Experiments on Air*; and had collected materials for a fourth, which he published soon after settling at Birmingham. Here, also, he published his attack upon Drs.

Reid, Beattie, and Oswald, a book which he tells us he finished in a fortnight; but of which he afterwards, in some measure, disapproved. Indeed, it was impossible for any person of candour to approve of the style of that work, and the way in which he treated Dr. Reid, a philosopher certainly much more deeply skilled than himself in metaphysics.

After some years, Lord Shelburne began to be weary of his associate; and expressing a wish to settle him in Ireland, Dr. Priestley, of his own accord, proposed a separation, to which his Lordship consented, after settling on him an annuity of 150l., according to a previous stipulation. This annuity he continued regularly to pay during the remainder of the life of Dr. Priestley.

His income being much diminished by his separation from Lord Shelburne, and his family increasing, he found it difficult now to support himself. At this time Mrs. Rayner made him very considerable presents, particularly at one time a sum of 400*l.*; and she continued her contributions to him almost annually. Dr. Fothergill had proposed a subscription, in order that he might prosecute his experiments to their utmost extent, and be enabled to live without sacrificing his time to pupils. This he accepted: it amounted at first to 40l. per annum, and was afterwards much increased. Dr. Watson, Mr. Wedgewood, Mr. Galton, and four or five more, were the gentlemen who joined with Dr. Fothergill in this generous subscription.

Soon afterwards he settled in a meeting-house at Birmingham, and continued for several years keenly engaged in theological and chemical investigations. His apparatus, by the liberality of his friends, had become excellent; and his income was so good that he could prosecute his researches to their full extent. Here he published the last three volumes of his *Experiments on Air*, and many papers on the same subject in the Philosophical Transactions. Here, too, he continued his theological repository, and published a variety of tracts on his peculiar opinions in religion, and upon the history of the primitive church. Here he unluckily engaged in controversy with the established clergy of the place, and expressed his opinions on political subjects with a degree of freedom, which, though it would have been of no consequence at any former period, was ill suited to the peculiar times which were introduced by the French revolution, and to the political maxims of Mr. Pitt and his administration. His answer to Mr. Burke's book on the French Revolution excited the violent indignation of that extraordinary man, and he inveighed against his character repeatedly, and with particular virulence, in the House of Commons. The clergy of the Church of England too, who began about this time to be alarmed for their establishment, of which Dr. Priestley was the open enemy, were particularly active. The press teemed with their productions against him, and the minds of their hearers seem to have been artificially excited. Indeed, some of the anecdotes told of the conduct of the clergy of Birmingham were highly unbecoming their character. Unfortunately Dr. Priestley did not seem to be aware of the state of the nation, and of the plan laid down by Mr. Pitt and his political friends; and he was too fond of controversial discussions to yield tamely to the attacks of his antagonists.

These circumstances seem in some measure to explain the disgraceful riots which took place in Birmingham in 1791, on the day of the anniversary of the French Revolution. Dr. Priestley's meeting-house, and his dwelling-house, were burnt; his library and his apparatus destroyed; and many manuscripts, the fruits of several years of industry, were consumed in the conflagration. The houses of several of his friends shared a similar fate, and his son narrowly escaped death, by the care of a friend who forcibly concealed him for some days. Dr. Priestley was obliged to make his escape to London; and a seat was taken for him in the mail-coach under a borrowed name. Such was the ferment against him, that it was believed he would not have been safe any where else; and his friends would not allow him, for several weeks, to walk through the streets.

He was invited to Hackney to succeed Dr. Price in the meeting-house of that place. He accepted the office; but such was the apprehension of his unpopularity, that nobody would let him a house, apprehensive that it would be burnt by the populace as soon as it was known that he inhabited it. He was obliged to get a friend to take a lease of a house in another name; and it was with the utmost difficulty he could prevail on the landlord to allow the lease to be transferred to him. The members of the Royal Society, of which he was a fellow, declined admitting him into their company; and he was obliged to withdraw his name from the Society. His sons, disgusted with this persecution of their father, had renounced their native country, and gone over to France; and on the breaking out of the last war they emigrated to America. It was this circumstance, together with the state of insulation in which he lived, that induced Dr. Priestley, after much consideration, to form the resolution of following his sons, and emigrating to America. He published his reasons in the preface to a Fast day sermon, printed in 1794, one of the gravest and most forcible pieces of composition I have ever read. He left England in April 1795, and reached New York in June. In America he was received with much respect and attention by all ranks; and was immediately offered the situation of Professor of Chemistry in the College of Philadelphia: which, however, he declined, as his circumstances, by the liberality of his friends in England, continued independent.

He settled finally in Northumberland, about 130 miles from Philadelphia; where he built a house, and re-established his library and laboratory. He published a considerable number of chemical papers, some of them in the form of pamphlets, and the rest in the American Transactions, the New York Medical Repository, and Nicholson's Journal. Here, also, he continued keenly engaged in theological pursuits; and published, or re-published, a great variety of books on religious subjects. Here he lost his wife, and his youngest and favourite son, who, he had flattered himself, was to succeed him in his literary career; and here he died, in 1804, after being only two days confined to bed, a few hours after he had arranged all his literary concerns, inspected some proof-sheets of his last theological work, and given instructions to his son how it should be printed.

During the latter end of the presidency of Mr. Adam, the same kind of odium which had banished Dr. Priestley from England began to prevail in America: he was threatened to be sent out of the country as an alien. Notwithstanding this, he declined being naturalized; resolving, as he said, to die, as he had lived, an Englishman. When his friend Mr. Jefferson, whose political opinions coincided with his own, became President, the odium against him wore off, and he became as much respected as ever.

As to the character of Dr. Priestley, it is so well marked by his life and writings, that it is difficult to conceive how it could be mistaken by many eminent literary men in this kingdom. Industry was his great characteristic; and this, together with an extreme facility of composition, acquired, as he tells us, by a constant habit, while young, of drawing out an abstract of the sermons which he heard preached, and by writing a good deal in verse—these two qualities it was that enabled him to do so much: yet he informs us that he never was an intense student, and that his evenings were usually passed in amusement or company. He was an early riser; and always lighted his own fire before any one else was stirring: it was then that he composed almost all his works. It is obvious, from merely glancing into his books, that he was precipitate; and indeed, from the way he went on, thinking as he wrote, and writing only one copy, it was impossible he could be otherwise. But as he was perfectly sincere, and anxious to obtain the truth, he freely acknowledged his mistakes as soon as he became sensible of them. This is very visible in his philosophical investigations; but in his theological writings it was not so much to be expected. He was generally engaged in controversy in theology; and his antagonists were often insolent, and almost always angry. We all know the effect of such an opposition; and need not be surprised that it operated upon Dr. Priestley, as it would do upon any other man. By all accounts, his powers of conversation were very great, and his manners in every respect extremely agreeable. That this must have been the case is obvious, from the great number of his friends, and the zeal and ardour with which they continued to serve him, notwithstanding the obloquy under which he lay, and even the danger that might be incurred by appearing to befriend him. As to his moral character, even his worst enemies have been obliged to allow that it was unexceptionable. Many of my readers will, perhaps, smile, when I say that he was not only a sincere but a zealous christian, and would willingly have died a martyr to the cause: yet I think the fact is undoubted; and his conduct through life, and especially at his death, affords irrefragable proofs of it His tenets, indeed, did not coincide with those of his country: but though he rejected many of the doctrines, he admitted the whole of its sublime morality, and its divine origin; which, in my opinion at least, is sufficient to constitute a true christian. His manners were perfectly simple and unaffected: and he continued all his life as ignorant of the world as a child. Of vanity he seems to have possessed more than a usual share; but, perhaps, he was rather deficient in pride.

Let us now take a view of the writings and opinions of Dr. Priestley, arranging our observations under the four grand heads of Philosophy, Theology, Metaphysics, and Politics.

His philosophical writings claim the first rank, because to them he was indebted for his reputation, an dbecause upon them, I am persuaded, his reputation will ultimately rest. His philosophical writings are the following: 1. His History of Electricity. 2. The History of the Discoveries relative to Light and Colours. 3. An Introduction to Electricity. 4. An Introduction to Natural Philosophy. 5. A Treatise on Perspective. 6. His Experiments on Air: published successively in six volumes octavo; and afterwards new modelled and compressed by him into three volumes. Besides these a considerable number of papers on electricity and chemistry were published separately, either in the form of pamphlets, or in the Philosophical Transactions, or other periodical works. All his chemical labours finished after his arrival in America were published in this way.

His History of Electricity was the work which first gave him celebrity, and introduced him to the numerous philosophers who at that time were devoted to electrical pursuits. It was written in less than a year, and published in 1767. It went pretty rapidly through three editions. The books requisite were supplied by Dr. Franklin, who was at the pains to read and correct every part of the manuscript as it was transcribed. I cannot say that this work appears to me deserving of the great reputation which it acquired for its author. It is very nearly in the form of annals; the discoveries made by each author being arranged almost exactly according to the time in which he wrote. Dr. Priestley seems to have placed the books in chronological order, and to have read and abridged them in that order, and never afterwards to have altered what he wrote. Now in giving an account of a science, it is very seldom that the chronological order is the best suited to the detail of facts. Some of the first principles are usually very late in making their appearance. Hence many of the early discovered facts receive their interest and explanation from facts discovered long afterwards; and unless the whole be arranged so as to make the facts bear upon each other, it is to a great degree destitute of interest. I confess that this was the feeling which attended my first perusal of the History of Electricity. The book, however, possesses value; as it collects in one view the scattered facts which were spread through a great variety of books which preceded him, and which at that time it was difficult to obtain. The great eclat which Electricity had at that time acquired, owing in a great measure to the very splendid experiments made by the Royal Society, chiefly I believe in consequence of the influence of Dr. Watson, the discovery of the identity of lightning and electricity by Dr. Franklin, and the discovery of the Leyden phial and its wonderful effects by Muschöenbroek—the great eclat produced by these splendid discoveries gave an unprecedented popularity to electrical pursuits. Every body was anxious to be acquainted with the facts; and as no treatise on the subject had appeared at all comparable to that of Dr. Priestley, we need not be surprised that it became popular, and that the reputation of its author suffered a corresponding exaltation; especially as his book contained several original experiments, and as he continued to prosecute the subject for some years afterwards. Dr. Priestley led to various important discoveries in electricity, by introducing a change in the apparatus, substituting powerful machines and large batteries to the very puny ones formerly in use. Mr. Nairne's machine, by far the simplest and best in use, before the introduction of the plate ones by Cuthbertson, was in some measure owing to Dr. Priestley, as he suggested changes and improvements upon which that consummate artist went to work, and soon brought the machine to the requisite simplicity.

The principal discoveries in electricity made by Dr. Priestley were the two following: 1. Charcoal is a perfect conductor of electricity. 2. All metals, without exception, may be oxydized by passing through them a sufficiently strong electrical charge. He made several other minor discoveries; but in this general sketch, I do not think it worth while to enumerate them. To the theory of electricity he made no additions nor improvements whatever. Indeed, his turn of mind, and his precipitancy, did not fit him well for theorizing. It is to Franklin and Cavendish that we are chiefly indebted for the electrical theory, such as it is. Epinus also published an excellent work on the subject, which Dr. Priestley must have seen, as he quotes it; but his mathematical knowledge, I presume, was not sufficient to enable him to understand it. Some important additions might now be made to the theory, in consequence of the discoveries of Volta and Davy; but several points still remain to be cleared up, and they are involved in considerable difficulty. Some of the French writers, particularly Haüy, still adhere to the old opinion of two electrical fluids; but in this country every body admits only one fluid. Till lately I considered our hypothesis as far preferable to that of the old theory of Du Faye; but some of Sir H. Davy's experiments give plausibility to the opposite opinion. This, however, is not the place for enlarging on these difficult and abstruse points, concerning which, I fear, absolute certainty is entirely out of the question. The improvements in electricity, since Dr. Priestley's book appeared, are so numerous and important, that his work in no degree represents the present state of the science: a new history would be requisite.

His History of the Discoveries relative to Light and Colours was published in 1772. It was a much more difficult task than the former, and was by no means so successful. It never came to a second edition; and indeed, Dr. Priestley informs us that it cost him a considerable sum of money. The Doctor was by no means fitted for this task. A great part of the subject was mathematical; and controversies had taken place in it, which it was scarcely possible to understand, without a much greater portion of mathematical knowledge than he possessed. Besides, a mathematical subject was much less suited to the characteristic rapidity of Dr. Priestley, who could not peruse the books on the subject in a cursory manner, and was therefore obliged to slur them over altogether. I do not mean to deny that the book possesses considerable value; but had the author not distinguished himself in other departments, this treatise would not have been sufficient to have brought him into notice.

As to his Elementary Treatise on Electricity and Natural Philosophy, and his book on Perspective, I have never had an opportunity of perusing any of them, and cannot therefore give any account of them. They are said to be written in a lively manner, to be very entertaining, and well calculated for enticing young men to the respective studies.

We come now to his chemical labours, to which he was chiefly indebted for the great reputation which he acquired. No man ever entered upon any undertaking with less apparent means of success than Dr. Priestley. He was unacquainted with chemistry, excepting that he had some years before attended an elementary course delivered by Mr. Turner, of Liverpool. He was not in possession of any apparatus, nor acquainted with the method of performing chemical experiments: and his circumstances were such that he could neither lay out a great deal of money on experiments, nor could he hope, without a good deal of expense, to make any great progress in his investigations. These circumstances, which at first sight seemed so adverse, were I believe of considerable service to him, and contributed very much to his ultimate success. The branch of chemistry which he selected was new. An apparatus must be invented before any thing of importance could be effected; and as simplicity is essential in every apparatus, he was likely to contrive the best, whose circumstances obliged him to attend to economical considerations.

Though the investigation of the properties of air had been prosecuted with considerable success by Boyle; and though Hales had demonstrated the possibility of extracting air from a great variety of substances by very simple processes; and though Dr. Black had ascertained that the *air* which exists in limestone, and which is separated during the fermenting of malt liquors, possesses properties different from *common air*; it can scarcely be said that pneumatic chemistry was properly begun till Mr. Cavendish published his valuable paper on carbonic acid and hydrogen gas in the year 1766. He first pointed out the method of examining the properties of gaseous bodies, and invented an apparatus for the purpose, nearly similar to one employed by Mayow about a century before; but of the existence of which Mr. Cavendish was ignorant. Dr. Priestley was the inventor of the apparatus still used by chemists in pneumatic investigations. It is greatly superior to that of Mr. Cavendish; and indeed as convenient as can well be desired. Were we indebted to him for nothing else than this apparatus, it would deservedly raise his name very high.

The discoveries of Dr. Priestley in pneumatic chemistry are so numerous that I cannot even attempt a bare enumeration of them; but must rest satisfied with a general outline. His first paper was published in 1772. It was on the method of impregnating water with fixed air. The experiments contained in it were made in consequence of his residing near a brewery at Leeds. This accidental position, therefore, was in some measure the cause of his beginning his great chemical career. This pamphlet was immediately translated into French; and at a meeting of the College of Physicians in London, they addressed the Lords of the Treasury, pointing out the advantage that might result from water impregnated with fixed air, according to Dr. Priestley's method, in cases of the scurvy at sea. His next essay was published in the Philosophical Transactions, and procured him the Copleyan medal. His different volumes on air were published in succession, while he lived with Lord Shelburne, and while he was settled in Birmingham. They drew the attention of all Europe, and raised the reputation of this country to a very great height.

The first of his great discoveries was *nitrous gas*, which had indeed been formed by Dr. Hales; but that philosopher had not attempted to investigate its properties. Dr. Priestley ascertained its properties with great sagacity, and almost immediately applied it to the analysis of air. It contributed prodigiously to all subsequent investigations in pneumatic chemistry, and in some measure led to our present knowledge of the constitution of the atmosphere. The next grand discovery was *oxygen gas*, to which we are indebted for the revolution which chemistry soon after underwent. This substance, however, would have been discovered independent of Dr. Priestley; for Scheele announced it, and founded on it an analysis of air; and there is every reason to believe that he was ignorant of what Dr. Priestley had done, though his book was not published till three years after Dr. Priestley had actually announced his discovery to the public. Lavoisier, likewise, laid claim to the discovery of oxygen gas; but his claim is entitled to no attention, and is inconsistent with that candour which he displayed on other occasions; for Dr. Priestley prepared oxygen gas in Mr. Lavoisier's house in Paris, and showed him the method of procuring it during the year 1774, a considerable time before Lavoisier's pretended discovery was made.

To Dr. Priestley we are likewise indebted for the discovery of most of the other gaseous bodies at present known, and for the investigation of their properties. Indeed, carbonic acid, hydrogen, sulphureted and phosphureted hydrogen, and oxymuriatic acid, are almost the only ones for which we are not indebted to him. Sulphurous acid, fluoric acid, muriatic acid, ammoniacal, carbureted hydrogen, carbonic oxide, and nitrous oxide gases, were all first produced and investigated by him. Though he did not discover hydrogen gas, yet his experiments on it were highly important, and contributed essentially to the revolution brought about in chemistry. Nothing, for example, could be more striking than the reduction of oxide of iron, and the disappearing of hydrogen when the oxide (or finery cinder, for it was that which he used) is heated sufficiently in contact with hydrogen gas. Azotic gas was discovered before he began his career; but we are indebted to him for most of the properties of it yet known. To him also we owe the first knowledge of the acid produced when the electric spark is taken for some time in common air; a fact which led afterwards to the knowledge of the constituents of nitric acid, which contributed so essentially to the establishment of the new chemical doctrine. He first discovered the great increase of bulk which takes place when electric sparks are passed through ammoniacal gas; a fact which led directly to the analysis of ammonia by Berthollet, who merely repeated Priestley's experiments. His curious experiments on the freezing of water, and the unlimited production of azote from it, remain still unexplained; and probably will not be understood till some person has succeeded in decomposing azote, and ascertaining its constituents. His experiments on the amelioration of atmospherical air by the vegetation of plants, on the oxygen gas given out by them in the sun, and on the respiration of animals, are no less curious: but it would be improper to enter in this place on these difficult and still disputed subjects.

Priestley may be considered in some measure as the pioneer of Lavoisier. This sagacious philosopher availed himself of all the discoveries of the former, repeated and arranged them, and by means of them chiefly, and of the discoveries of Mr. Cavendish, he succeeded in establishing his peculiar opinions. Priestley continued an advocate for the phlogistic theory till the end of his life; and, the year before his death, published a curious paper, in which he summed up all his objections to the Lavoisierian theory. Many of these objections are easily answered; but there are others which it is impossible to explain. Indeed, the subject cannot be considered as fully decided at present. Lavoisier's theory is more elegant and simple than the other; but all the chemical facts known even at present are susceptible of an explanation, according to the doctrine of Dr. Priestley. It would scarcely be fair, therefore, to say that his doctrine is refuted. We prefer the Lavoisierian explanation, because it is simpler and easier than the other; but it may probably be very different from the theory which will ultimately prevail. Indeed, it is easy to foresee the risk that the Lavoisierian theory runs at present, from the new opinions of Davy respecting muriatic and fluoric acids. The discovery of the prodigious effect produced on the qualities of bodies by the presence or absence of electricity, a substance (if it be a substance) of so subtile a nature as to produce no effect upon the most delicate balance, and the great changes produced by minute additions of powderable matter, alter all our notions of the composition of bodies. If mercury, for example, can be made solid, and reduced to one-third of its specific gravity, by the addition of one 100,000th part of its weight of the metal of ammonia (as is the opinion of Berzelius), how can we be certain that the substances which we find in analysing bodies constitute even the most material part of their composition. A great deal will depend upon the nature of *azote*, a substance obviously a compound, though nobody hitherto has been able to decompose it. I should not, for my part, be surprised to see some of the opinions of Dr. Priestley, which were looked upon as most absurd—as, for example, that all combustible bodies contain a common principle of inflammability—established in a satisfactory manner.

The next grand point of view in which we are to examine Dr. Priestley, is as a theologian. Theology was the great object of his life; and the writings which he published on the Christian religion are so numerous, that I could not attempt to enter upon any analysis of them: but as his religious opinions were peculiar, and very well marked, I shall endeavour to give an abstract of his creed.

He was educated in the principles of strict Calvinism, to which all his early friends were zealously attached, and from which, therefore, he could not deviate without injuring himself materially in their good opinion. He first became an Arminian, then an Arian, and last of all a Socinian. As might have been expected from his situation, he was an enemy to all church establishments, and thought that there should be no connection between church and state.

He believed in the existence of one God, infinite in power, in wisdom, and in goodness, the author and creator of all things. The system of the universe was, in his opinion, the best possible; the apparent imperfections and the evil which exists in it being necessary to produce the greatest possible quantity of happiness. The object of the Deity was to communicate happiness to all his creatures, an object which will be ultimately accomplished, and all creatures of course will be ultimately happy. The object of punishment, when the Deity inflicts it on his creatures, is not vengeance, but reformation; and when it has accomplished that object, it will cease of itself. Things were arranged with such skill at the original creation, that there is no occasion for the interference of the Deity, except in particular cases allowed for at first: hence the doctrine of a particular providence was not admitted into his creed. For the knowledge of life and immortality in a future world we are entirely indebted to the Christian religion: natural reason might have made the opinion probable, but was not sufficient to show us that it was really true.

It was necessary for the Deity to send instructors occasionally to teach men a purer system of morality than they were naturally disposed to practise; and to destroy all tendency to polytheism, by inculcating the Unity of the Divine Being. Such instructors were Moses and Jesus Christ, who inculcated each the system of morality best suited to the particular times in which they lived. They were endowed with the power of working miracles, in order to prove the truth of their mission; and with superior knowledge, as far as their mission was concerned: but in other respects their knowledge did not exceed that of the age in which they lived. The Old and New Testaments contain the history of the Jewish and Christian dispensations, written by different persons, who had good information relative to the particulars related. The writers were not influenced by divine inspiration, but precisely in the same circumstances as other historians: liable, like them, to mistakes; and to be judged of by the same rules as those which we use in examining the writings of common historians. They occasionally contradict each other in a few things of minor importance, and in some cases relate circumstances in which they seem to have been misinformed; but upon the whole, the evidence for their veracity and fidelity is so strong, that it would be a greater miracle to admit the possibility of their accounts being forgeries, than to admit the truth of the Christian religion.

Christ was a mere man, commissioned by God, and capable of working miracles; but possessed of no superior knowledge to other men, except what related to his mission. The same remark applies to the Apostles, who sometimes reason illogically and inconclusively, especially the Apostle Paul, though in other respects a wonderful man. The account of the immaculate conception, and several other particulars in the history of our Saviour, were rejected as fabulous: and much of the reasoning of the Apostle Paul, in some of his most remarkable epistles, was rejected as inaccurate. The doctrine of original sin, of the atonement, of election and reprobation, and of the eternity of a future punishment, were rejected as absurd and unscriptural. The object of the mission of Christ was merely to teach the immortality of the soul, and to propagate a more perfect system of morality, and a more accurate estimate of the Divine attributes than mankind had possessed before. From his peculiar opinions respecting the materiality of the soul, Dr. Priestley refused to admit the doctrine of an intermediate state. Every man continues insensible till the time of the resurrection, when he rises as if from sleep, and is not sensible of the interval that has elapsed since his death.

Such is a very short outline of Dr. Priestley's ultimate creed. To enter into farther particulars would lead us into too wide a field. Hence I omit his historical disquisitions on the corruptions of Christianity, and the opinions of the early Christians; books which must be admitted to be inaccurate in several particulars, but which nevertheless contain much curious and valuable matter, not easily to be found any where else. Neither shall I attempt to point out the inconsistencies visible between some of his religious opinions, nor the danger of the liberty which he assumed of blotting out of the Scriptures every thing which appeared to him to be absurd or inconsistent with his peculiar creed. Were I to enter upon the subject, it would not be easily exhausted; but it is unnecessary, and would be on many accounts improper.

We come now to view Dr. Priestley as a metaphysician. Upon his metaphysical opinions, however, I consider it unnecessary to go into detail. He adopted Dr. Hartley's peculiar opinions as the basis of his whole doctrines; and was at the trouble to publish a kind of abstract of Dr. Hartley's book. He distinguished himself chiefly by adopting two metaphysical opinions, both of which procured him a good deal of obloquy and opposition. He denied the existence of an immaterial being or soul in man. Thinking, according to him, depends entirely upon organization. Man is merely material. When he dies, he dies completely, and continues insensible till the body is again restored, when he revives with the same sensations and information that he formerly possessed, and insensible of the interval which has elapsed since his death. He adopted the side of necessity in his disquisitions, on the long agitated question of Liberty and Necessity, and supports his opinion by nearly the same arguments as those advanced by Mr. Cowper and Dr. Crombie in their subsequent essays on the same subject. For my part, I consider this celebrated question as little more than a verbal dispute. Both sides seem to mean precisely the same thing, though they express themselves in a different manner. As to Dr. Priestley's answer to the Scotch metaphysicians, and his refutation of their doctrine of common sense, I do not think it necessary to enter upon the subject. Every body is of opinion, I believe, that this refutation, at least as far as Dr. Reid is concerned, was incomplete; and that the style of writing in which Dr. Priestley indulged was highly indecent and improper: indeed, he himself afterwards confessed as much.

Let us in the last place take a view of the political principles of Dr. Priestley, and see whether they will account for that obloquy and persecution to which he in some measure fell a victim.

I may remark, in the first place, that he was an advocate for the perfectibility of the human species, or at least its continually increasing tendency to improvement: a doctrine extremely pleasing in itself, warmly supported by Franklin and Price, and which the wild principles of Condorcet, Godwin, and Beddoes, have at last brought into discredit. This doctrine was taught by Priestley in the outset of his Treatise on Civil Government, first published in 1768. It is a speculation of so very agreeable a nature, so congenial to our warmest wishes, and so flattering to the prejudices of humanity, that one feels much pain to be obliged to give it up. Perhaps it may be true in a limited sense, though the very impressive and convincing reasoning of Malthus, the past history of mankind, and I fear, too, the present aspect of Europe, are much more favourable to the notion, that mankind are doomed to travel in periodical circles of improvement and retrogradation. Perhaps it may be true, and I am willing to hope so, that improvements once made are never entirely lost, unless they are superseded by something much more advantageous; and that therefore the knowledge of the human race, upon the whole, is progressive: but political establishments, at least as far as we can judge from the past history of mankind, have their uniform periods of progress and decay. Nations seem incapable of profiting by experience, and cannot therefore make much progress except by mere accident. Every nation seems destined to run the same career, and the history may be comprehended under the following words: Poverty, Liberty, Industry, Power, Wealth, Dissipation, Anarchy, Destruction. And when a nation has once run this career, it seems incapable of renovation: virtue once destroyed can never be renewed.

Dr. Priestley's short Essay on the First Principles of Civil Government was first published in 1768. In it he lays it down as the foundation of his reasoning, that "it must be understood, whether it be expressed or not, that all people live in society for their mutual advantage: so that the good and happiness of the members, that is, the majority of the members of any state, is the great standard by which every thing relating to that state must be finally determined; and though it may be supposed that a body of people may be bound by a voluntary resignation of all their rights to a single person, or to a few, it can never be supposed that the resignation is obligatory on their posterity, because it is manifestly contrary to the good of the whole that it shall be so." From this first principle, which for my own part I consider reasonable, and even self-evident, he deduces all his political maxims. Kings, senators, and nobles, are merely the servants of the public; and when they abuse their power, in the people lies the right of deposing, and consequently of punishing them. He examines the expediency of hereditary sovereignty, of hereditary rank and privilege, of the duration of parliament, and of the right of voting, with an evident tendency to republican principles, though he does not express himself very clearly on the subject. Such appear to have been his political principles in 1768, when his book was published. It excited no alarm, and drew but little attention. These principles he retained ever after; or indeed he may be said to have become more moderate instead of violent. Though he approved of a republic in the abstract, yet considering the habits and prejudices of the people of Great Britain, he laid it down as a principle that their present form of government was best suited to them. He thought, however, that there should be a reform in parliament, and that parliaments should be triennial instead of septennial. He was an enemy to all violent reforms, and thought that they ought to be brought about gradually and peaceably. When the French revolution broke out, he took the side of the patriots, as he had done during the American war, and wrote a refutation of Mr. Burke's extraordinary performance. Being a Dissenter, it is needless to say that he was an advocate for complete religious freedom. He was ever an enemy to all religious establishments, and an open enemy to the church of England.

Such, as far as I have been able to collect them, were the political tenets of Dr. Priestley. How far they were just and right, I shall not take upon me to say; but that they were perfectly harmless, and that many other persons in this country for the last hundred years have adopted similar sentiments, without incurring any odium whatever, or without exciting the jealousy, or even the attention of government, is well known to every person. It becomes then a question of some curiosity at least to what are we to ascribe the violent persecution raised against him. It seems to have been owing chiefly to the alarm caught by the clergy of the Church of England, that their establishment was in danger: and considering the ferment excited soon after the breaking out of the French revolution, and the rage for reform which seemed to have pervaded all ranks, perhaps this alarm was not entirely without foundation. Though I can scarcely allow myself to think that there was occasion for the violent alarm caught by Mr. Pitt and his political friends, and the very despotic measures which they adopted in consequence. The disease would probably have subsided of itself, or it would have been cured by a much gentler treatment. I have seen a patient labouring under a pleurisy deprived of 120 ounces of blood in two days, and by that means very speedily cured of his disease, but reduced at the same time to such a state of debility that death was to be dreaded from that cause alone. I have seen another patient labouring under the same disease to the same extent, bled much more sparingly, and yet recover as rapidly as the other; and when the disease was got rid of, able in a few days to resume his usual employment. Mr. Pitt approved and followed the violent system of cure; but the lenient would have been equally efficacious, and less hazardous. As Dr. Priestley was an open enemy to the Church of England establishment, its clergy naturally conceived a prejudice against him; and this prejudice was violently inflamed by the danger to which they thought themselves exposed. Their influence with the ministry was very great, and Mr. Pitt and his friends naturally caught their prejudices and opinions. Mr. Burke, too, who had changed his political principles, and who was inflamed with the burning zeal which distinguishes all converts, was provoked at Dr. Priestley's answer to his book on the French revolution, and took every opportunity to inveigh against him in the House of Commons. The conduct of the French, likewise, who made Dr. Priestley a citizen of France, and chose him a member of their Assembly, though intended as a compliment, was injurious to him here. It was laid hold of by his antagonists to convince the people that he was an enemy to this country; that he had abjured his rights as an Englishman; and had adopted the principles of the hereditary enemies of Great Britain. These causes, and not his political opinions, appear to me to account for the persecution which was raised against him. This persecution is deeply to be lamented, as it interrupted his pursuits, destroyed the fruits of several years of labour, and drove him to a country where it was out of his power to pursue his scientific experiments with the same advantage as he would have done at Birmingham. We have been deprived, in consequence, of many important and curious discoveries, which he would have brought to light had he been left unmolested. Perhaps, however, the change was in reality of advantage to his reputation. He had carried his peculiar researches nearly as far as they could go. To arrange and methodize them, a different branch of chemical science was necessary, which he had not cultivated, and which his characteristic rapidity rendered it difficult for him to cultivate. It is possible, therefore, that by prosecuting his labours he might have injured his reputation, instead of promoting it, and might have lost that eminent situation as a man of science which he had so long occupied.

Article II.

Memorandums respecting some Minerals from Greenland. By Thomas Allan, Esq.; Edinburgh.*

On a former occasion I submitted to the Society some observations on a mineral which I supposed to be crystalized gadolinite. Since that time I have had an opportunity of obtaining the only unequivocal test by which the real nature of any new or uncertain fossil can be known. Dr. Thomson kindly undertook the chemical investigation of it; and has had the satisfaction to discover this mineral to be a substance entirely new. The result of his labours he communicated to the Society, together with the analysis of another new mineral found in the same parcel. I

* Read by Mr. Allan before the Royal Society of Edinburgh on the 20th April, 1812.

have since thought that it might prove interesting to mineralogists to know by what minerals these substances were accompanied, and have accordingly drawn up the following account, from which some idea may be formed of the great variety of minerals that are to be found in the almost unknown regions of Greenland. From the very uncouth and careless manner in which the collection appeared to have been formed, I was at first led to suspect that it could not have been made by any person of science. In this, however, I was mistaken, having subsequently learnt that the collector is a German mineralogist of high repute, of the name of Giescké. Having heard of the fate of his minerals, soon after they were captured, with an enthusiasm much to be admired, being still in Greenland, I am informed he immediately employed himself in repairing the loss by forming another collection. The return of Mr. Giescké to Europe is expected next spring; and as we have reason to hope that he will arrive at the port of Leith, we may be able to obtain farther elucidation on several interesting points, particularly the geognostic relations of some of the fossils, and also the geographic account of the whole, as at present we only know that the collection in question was captured in the Der Frechling, Captain Ketelson, on her passage from Greenland to Copenhagen. These minerals filled no less than nine or ten old boxes and barrels: a few specimens were wrapped in coarse paper; but a scanty supply of dry meadow moss was the only other material with which they were prevented from injuring each other. Before I examined them they were turned out on the floor of a merchant's warehouse in Leith, and lay such a spectacle of uninviting rubbish, that they were thought wholly unworthy of attention by all those who had previously seen them: which principally arose from the very great quantity of rubbish and water-worn stones, many of them covered with marine insects, with which the collection was loaded. The impression their first appearance made upon me was entirely similar, till my attention was attracted by some large white masses of what I thought resembled cryolite, of which I had obtained a few grains when in Paris as a present of a great value. On closer inspection I soon found I was right; while, most fortunately for me, the masses alluded to passed with others as sulphate of lime. Indeed, the very circumstance of its abundance was enough to stifle any suspicion of its being the rare and sought for mineral. I consequently accomplished the purchase, along with my friend Col. Imrie, without opposition; and having caused the minerals to be washed, in order to remove the dirt and soil with which they were covered, I carefully examined each separate specimen, and after throwing aside about one half of the entire bulk as useless, the remainder turned out to be of much more value than I at first expected. Without entering into a detailed mineralogical description of each variety, I shall now briefly enumerate the minerals contained in this collection, mentioning only such peculiarities as appeared to me interesting, or that have hitherto escaped notice.

Cryolite.

It is somewhat remarkable that on its first introduction into Europe, this mineral met with the fate it so narrowly escaped in this country. It has so strong a resemblance to some of the most common fossils, that when received at Copenhagen it was thrown aside as sulphate of barytes, and remained neglected for some years. Its inferior specific gravity at last attracted the notice of Abildgaard, who detected the presence of fluoric acid and alumine in its composition; the former of which had till then only been met with in combination with lime. The subsequent investigations of Klaproth and Vauquelin have given us the analysis of this stone in a more perfect form. Besides fluoric acid and alumine, they found soda, forming about one-third of the whole. Thus adding to the catalogue of minerals the most interesting compound the kingdom affords. The name which was given to it by Abildgaard, was suggested by its wonderful fusibility, in which respect it surpasses every other mineral. When in a pure state the colour is milk white, and from its transparency it sometimes presents a greyish tinge, particularly after immersion in water. I found it also of a brown colour, occasioned by an admixture of ferruginous matter, an accidental or mechanical combination, in which it had not been previously known: neither had it been described as being accompanied by any other substance, although I now found it along with sparry iron ore, galena, pyrites, quartz, and felspar. Of these the first was the most abundant, and occurred in larger groupes, presenting crystals of two inches in length by one in breadth. In the interior of some of this substance I observed minute transparent crystals; but in general they are of a deep blackish brown, and very much tarnished: some rhombs I found, detached and imbedded in the cryolite.

Although there were several masses accompanied with galena disseminated, there was only one specimen in which this mineral was crystalized: in this it presented a cube of about half an inch. The pyrites was likewise disseminated and crystalized, in the form of a cubo-dodecahedron. The quartz and felspar occurred in imbedded crystals. Hence all the substances which I found accompanying cryolite are crystalized; but I could discover no trace of any such arrangement in the substance itself, excepting in its threefold cleavage. Among all the other stones of which the collection consisted, I observed only two specimens accompanied by sparry iron ore unconnected with cryolite. These were disposed on sienite; but although the aspect of this fossil is exactly the same, it would be stretching analogy too far to assert from this that sienite was the geognostic repository of cryolite.

This substance obtained a very high estimation in the mineral market. I have in my possession a small specimen for which a friend of mine paid four pounds; and taking its weight and the price as the ratio, the value of all the cryolite in this parcel would have exceeded the sum of 5000*l.*

Apatite.

This mineral I found in three different states: first in grains of a pale greenish yellow colour, mixed with lamellar hornblende and magnetic iron ore; a combination which occurs in the iron mines of Lapland and Norway. The second is in transparent colourless crystals, of an hexagonal form, with terminations set at right angles to the axis, accompanied with dark brown mica and augite, both crystalized; and also with calcareous spar, in which the apatite was imbedded. In the third the apatite is of a greenish colour, somewhat similar to the moroxite of Kongsberg, and crystalized in short hexagonal prisms, imbedded, along with crystals of felspar, in a base of brick-red compact felspar, forming a species of porphyry hitherto unknown. There was only one specimen of this curious rock in the collection. The only other acidiferous minerals were carbonate of lime and fluor spar: of the former I found only one crystalized piece, and one or two of a fine white granular variety, similar to the marble of Carrara. Of the fluor I noticed one small mass of a purple colour, and some pale green pieces accompanying augite and felspar.

Quartz.

This is too common a substance to make it necessary for me to say much here: it however occurred in some unusual varieties. The most conspicuous was of a very common kind, although to appearance the specimens were more carefully chosen and preserved than any of the other minerals. The masses are of a flat shape, formed of a congeries of crystals, joined laterally, and projecting on each side, where the terminations are transparent, while the body is opake and white. Another variety consisted of thin brittle plates, disposed side by side, each apparently single plate being divided in two by an interstice not wider than the thickness of a sheet of paper, having the internal surfaces covered with extremely minute crystals. These masses have been exposed to the action of the weather, as in some places the plates are partly separated; so that, when pressed between the fingers, they crack and break like thin shells. I likewise found some pieces of rose-coloured quartz, and a variety similar to that which occurs at Bere Alstone in pseudo crystals, but not quite so fine in the grain. I did not observe any of the other varieties of the quartz family, such as agate or calcedony, if I except some minute particles of carnelian found among granular magnetic iron.

Zircon

Occurs in minute crystals imbedded in felspar along with, and also implanted in, allanite, for which substance I originally mistook it. It occurs in six-sided prisms terminated by pyramids of four sides: it is of a very dark colour, with smooth brilliant surfaces.

Garnet.

This substance I found of a form which I have not seen elsewhere: it is a regular octohedron, truncated on all the edges and angles. The principal crystal measures an inch and a quarter along the edge: it belongs to a group which had been imbedded in sodalite, and accompanied with augite. I found another variety of a dark olive green colour, crystalized in the leucite shape, and imbedded in bluish grey quartz; also the red transparent variety, presenting the same form, and imbedded in a compound of quartz and felspar; likewise in amorphous transparent concretions, imbedded in coarse gneiss; and, lastly, in small transparent grains, of a brilliant red colour, imbedded along with augite in snow-white granular felspar, forming one of the most beautiful rocks I ever beheld.

Felspar.

Besides as an ingredient in different rocks, I found this mineral separately, in great abundance, crystalized, laminated, compact, and granular. Of the first, the most remarkable variety is one that forms an ingredient in a species of trap. The crystals were principally detached; but some I found connected with that rock, the base of which is of a very coarse grain. They are so rude in their conformation, so very coarse in their texture, and so uneven on the surface, that it was some time before I recognized them as crystals. They vary in size from three inches in length to one, with the other dimensions in proportion, nearly equal to the felspar crystals in the coarsest granite. The most distinct form is a four-sided rectangular prism, acuminated by one plane, which measures with the two opposite sides 99° 41′ and 80° 19′ being the form *unitaire*, fig. 80 of Haüy. Some of these crystals are modified by the truncation of the acute solid angles; the rest are principally macles; but are all so much defaced by attrition, that it is nearly impossible to know what the forms are. In colour, these crystals are grey; they are mixed throughout with minute particles, probably decomposed hornblende.

Of the *lamellar felspar* there was a very great abundance, some of it equal in opalescence to the labradore, though not possessed of the same variety of colour. I found some small fragments of green felspar, like the Siberian; and of the compact variety there were two kinds, the translucent and opake. The colour of the first is greenish white, with a waxy, small splintery fracture: it has some resemblance to the Chinese felspar, of which the fine porcelain is fabricated. The compact variety is of a whitish green colour, slightly translucent on the edges with a small grained splintery fracture. The *granular* variety is a substance by no means common; it is of a brilliant white, with a crystaline texture, not unlike saline marble; but is distinguished from it, and from granular quartz, by the lamellated structure of the grains: it contains imbedded garnet and augite.

Sodalite.

This is one of the substances alluded to as having been analyzed by Dr. Thomson. I must not here neglect to acknowledge my obligations to Mr. Ekeberg, the distinguished chemist of Upsala, who also analyzed this mineral, and most obligingly communicated the result of his investigation, in a letter dated 4th August, 1810; with which the labours of Dr. Thomson afforded a striking coincidence, as the following comparative statement indicates:—

	Ekeberg.	Thomson.
Silica	36	38·52
Alumine	32	27·48
Soda	25	23·50
Mur. acid	6·75	3
Iron	·15	1
Water		2·10
Loss		1·70

The very uncommon proportion of soda which this mineral contains suggested the name to Dr. Thomson, by whom it has been so well described that I find nothing remaining to be added. There is one circumstance, however, which I cannot pass over here, particularly as it was not noticed by the Doctor, relative to a fugitive colouring, which I observed on breaking up the masses. On the fresh fractures I was very much surprised to find a beautiful rose or purplish pink colour, and the more so to observe, after laying some specimens by, that in the course of a few hours this lively tint wholly disappeared. I had occasion to

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make several observations of the same sort: at one time I broke a mass, and laid one portion under the rays of the sun, while the other was placed in the shade: the former was deprived of the red hue almost instantaneously, while the other retained it for some time. This induced me to try the effect of excluding the light: I accordingly wrapt up a mass, which after a period of three years retained its colour nearly as fresh as ever. Among the dodecahedral crystals of sodalite, I obtained some having six of the solid angles, replaced by planes of four sides indicating the cube.

Sahlite.

This mineral has lately been considered by Haüy as a variety of augite. In this collection it occurred massive; also combined with sodalite. It was by means of the acute discrimination of the Count de Bournon that the sahlite was discovered: like sodalite, it occurs in lamellated masses; its colour is white and greenish white, with a brilliant shining surface; it divides with facility into rectangular prisms, terminated by a plane which measures with the opposite sides 106° 15′ and 73° 45′. Parallel to the diagonal of the terminal faces, there are indications of natural joints by which the prisms may be divided vertically: for these observations I am indebted to Mons. de Bournon. This substance is sometimes so intimately blended with sodalite, that it is nearly impossible to discriminate them; a circumstance which is common with some other minerals.

Tourmaline.

Scarcely any cabinet is destitute of specimens of the fine crystals of tourmaline, which are found in Greenland: it is therefore surprising that in a collection so extensive there should have been only one or two masses, containing a few very minute crystals of this substance. They are well defined, however, and imbedded in large grained granite, composed of quartz, very white felspar, and light coloured mica. The large tourmaline is found in a mountainous district 80 miles inland from the settlement of Newhernhut; but as there are upwards of 30 colonies along the west coast of Greenland, it is possible that Mr. Gieseké did not visit them all.

Amphibole.

This mineral was rather abundant in the collection. I found some masses two or three inches in diameter, and one fragment presenting a rhomboidal prism of 124° 30′ and 55° 30′, being the primitive form. The most remarkable specimens were crystallized in prisms of six sides, grouped and solitary, and imbedded in a granite rock. I met with nothing to indicate a termination but from their dimensions, some of the prisms measuring four inches by one. The style of the rock in which they occurred must have been on a very magnificent scale.

Augite.

Although this be a very common mineral, as a constituent in trap rocks, it was known in a crystalized state only as a volcanic production, until found in the neighbourhood of Arundahl in Norway. In the present collection it occurred abundantly, and in different varieties: First, in slender four-sided prisms, measuring 88° and 92°, terminated by dihedral summits, along with crystalized felspar and some light green coloured fluor, on the surface of a porphyritic greenstone, the base of which is remarkably fine grained. Second, in prisms of six sides, being the above form, with the obtuse edges replaced by a plane: among these I found one small crystal presenting the prism form, which is the prism of 88° and 92°, terminated by summits set on the obtuse angle, obliquely to the axis. These are disposed on the surface of a granitic rock. Third, imbedded with garnet in granular felspar, as before noticed, and in sodalite blended with amphibole; also in delicate diverging fibres, forming stellated groupes of considerable size. In these specimens augite enters into the composition of a rock, which is entirely new to us: its geognostic relation, therefore, may be looked for with a considerable degree of interest.

Epidote.

There were a few specimens which I believe belong to this species; but without analyses, and in the absence of crystalization, or some other prominent character, many of the minerals which present a texture sometimes fibrous and sometimes otherwise, are not very easily discriminated. Those which I consider epidote are massive: one is composed of minute interlaced fibres, longitudinally streaked, of a deep clear pistaccio green, and accompanied with felspar and prehnite: another, not so dark in the colour, is almost granular, a few streaked fibres being only to be discovered by means of a lens: the third is a mass of very delicate fibres confusedly disposed, but distinctly visible in the interior of the stone, although the external surface be compact and earthy. This specimen was extremely tough, owing to its fibrous texture. Mr. Jameson observes that massive epidote occurs in beds in primitive mountains: the above specimens have much the appearance of such a locality, except the last, which is more of a kidney shape.

Prehnite.

This mineral I found in a rock similar to that of St. Christophe, in Dauphiné: it may also be observed, that in both, the epidote and prehnite occur together. There is no such combination in the trap rocks which contain prehnite in this country. That of Greenland appears to have occupied the interior of a vein along with calcareous spar, and is crystalized in minute but remarkably distinct crystals, presenting the rhomboidal and hexagonal table. I likewise observed specimens of stilbite, analcime, and chabasie; but all so much rubbed, that it was difficult to recognise them.

Mica.

Besides accompanying augite, as already mentioned, I found this substance in large dark olive-green plates, also of a bright emerald colour in minute laminæ in a bit of gneis.

In this collection there were no minerals belonging to the inflammable class, if I except plumbago, which has recently been transferred to it by Haüy: some of this substance is so pure in the grain, as to be capable of tracing lines upon paper with a clearness equal to that of Burrowdale; others were very coarse in the grain, and one I found intimately connected with granite.

METALS.

Of these I found the following varieties:—

Lead.—Sulphuret, accompanying cryolite, as already described.

Copper.—Grey sulphuret, blended with a small proportion of green carbonate.

Iron.—Magnetic, hematitic, pyrites, and sparry ore. The first occurred in sand, with particles of olivine, carnelian, and dark-coloured mica, in irregular masses, imbedded in a granitic rock along with amphibole, also massive blended with amphibole, and accompanied with apatite. The hematitic iron in its common state, and mixed in a stratified manner, with what appears to be ferruginous sandstone. The pyrites, and sparry iron ore, have already both been noticed as accompanying cryolite.

Tin.—Of this substance I found two or three specimens crystalized in quartz, and accompanied with a few specks of felspar. This differs from the Cornish tin by submitting to the blow-pipe without decrepitation.

Molybdena occurred in a few specimens composed of quartz, intimately blended with light greenish yellow chlorite, dispersed in very minute specks through the mass.

Yttrotantalite.—The collumbite of Hatchett is the same as this mineral without yttria. This substance is exactly similar in colour to the allanite; but its compact texture, and smooth shining conchoidal fracture, induced me to suspect some difference, which suspicion was strengthened by the specific gravity; this was 5·7604 at 60° of Fahrenheit; and which was finally confirmed by Dr. Wollaston, who found it to be yttro-tantalite, nearly pure. Besides this variety, I found a crystalized substance, which I believe to belong to the same; it occurs in minute regular octohedrons, imbedded in felspar: their fracture is conchoidal; lustre, somewhat metallic; and colour, bright orange brown. These external characters first induced me to consider this mineral as a variety of spinel; but I have changed this opinion, in consequence of an investigation of Dr. Wollaston on some small atoms I was enabled to detach, who informed me that yttria entered into its composition; but that the portion submitted to analysis was too small to complete the experiment. There was only one very small specimen in the collection; and so little attractive is its appearance, that it was upwards of four years in my possession before I examined it. The form of the crystals first attracted my attention; but they are so minute, and so thinly scattered in the matrix, that the whole would scarcely have sufficed for the subject of an analysis, minute as the portions are which are now required for that purpose.

Allanite.

The last metallic substance I have to mention is that which was mistaken by myself for gadolinite, and subsequently found to be an unknown compound by Dr. Thomson, to whose politeness I am indebted for the compliment he has paid me, of giving it to the world under the name of allanite; although, perhaps, the chemical appellation of brown oxide of cerium would have been fully as appropriate. I have now nothing to add to the description already published by Dr. Thomson, excepting that the same substance has been found among a parcel of minerals from the Mysore, yielding by analysis the same notable proportions. It is somewhat singular that a new fossil should be discovered, so nearly at the same time, among minerals from two quarters of the world so widely separated.

Aggregate Rocks.

I have before mentioned that the greatest part of this collection consisted of stones destitute of any interest: my present object is merely to mention a few of the most characteristic. Of *granite* there were several varieties; but the most singular was a compound of felspar and quartz, like the graphic granite; but in this the base was of a bluish grey colour, and the included crystals pure white. *Porphyry*, of a base of a dark brown compact felspar, containing sharp angled crystals of the same, of a light flesh colour; also a variety similar to the red compact felspar porphyry of the Pentland hills. *Greenstone*, of a very coarse grain, and *basalt* in small columnar irregular fragments, from one inch and a half to two inches in length; none of them exceeds an inch in thickness. The ends are coated with a vitreous covering, similar to what was obtained on the sides of Basaltic veins by Sir George Mackenzie and his friends in Iceland. I suppose from their appearance that they are portions of a very minute vein of basalt. *Pitchstone*; the colour of this substance is dark green; fracture small, conchoidal, and uneven. Through the mass are dispersed numerous circular spots, somewhat lighter in colour; from the centre of which, in a strong light, a fibrous radiation may be observed. I have met with an appearance analogous in Arran; but am not aware that any thing of the kind has been before noticed in any of the mineralogical works, although it be of a description calculated to create interest. *Sandstone* in slaty fragments of a dark ochry red colour, with whitish orbicular spots; also a lighter red compact variety, similar to some of our indurated sandstones. I likewise found some masses of brescia composed of angular fragments combined by a paste of crystaline quartz. The inclosed masses are pieces of sandstone.

I have now to enumerate the minerals above noted.

Cryolite	
Carbonate of lime	Crystalized
	Granular
Fluate of lime	
Phosphate of lime	Crystalized
	Granular
Quartz	Crystalized
	In plates
	Compact, and
	Rose coloured
Zircon	
Garnet	Precious
	Green and
	Common
Felspar	Crystalized
	Granular
	Lamellar
	Compact
	Green, and
	Opalescent
Sodalite	
Sahlite	
Tourmaline	
Amphibole	
Augite	Crystalized
	Fibrous

Epidote	
Prehnite	
Stilbite	
Chabasie	
Analcime	
Olivine	
Mica	
Plumbago	Granular, and Compact
Lead	Sulphuret
Copper	Grey sulphuret
	Green carbonate
Iron	Magnetic
	Hamatitic
	Pyritical and Sparry ore
Tin	
Molybdena	
Yttro tantalite..........	Amorphous
	Crystalized
Cerium	Brown oxide
Granite	Graphic, and Common
Porphyry	
Greenstone	
Basalt	
Pitchstone	
Sandstone	
Brescia	

The above list of species and varieties comprehends in number more minerals than one-half of all that Scotland affords, according to Professor Jameson's table of geognostic relations (Vol. III. p. 277). The filth to which they had been exposed, and the little care which had been taken in package, were equally calculated to deprive them of their peculiarities, and in several the characters were only to be detected by careful and minute investigation. I cannot help being surprised, that such an ill assorted collection should have been made by a mineralogist of Mr. Gieseké's character, and wonder what object he could have had in view, by loading it with the quantity of useless rubbish which I had to throw out. The number of minerals, however, which I have been enabled to pick from among it, is sufficient proof of the country in which they were found being one of the most interesting nature: for details respecting it we must wait the arrival of the collector, for which I have no doubt every mineralogist, as well as myself, will look with some degree of impatience.

Article III.

Lythrodes; a new Species of Mineral from the North. By D. L. G. Karsten.*

Among the magnificent mineral products of *Fridrichswarn*, in Norway, I have found a mineral, the properties of which will be better known by the following description of its characters:—

Colour. Aurora red, passing into brownish red: and in some specimens through flesh red into yellowish brown and pale brown: here and there with cream, yellow, and greenish spots.

External aspect. Massive and disseminated.

Lustre. In the principal fracture, resinous and glimmering; in the cross fracture, without lustre.

Fracture. Passing from uneven to splintery; but the *texture* concealed foliated with several cleavages, which were only ascertained after having examined a number of specimens.

Fragments show a tendency to regularity.

Distinct concretions. Granular, with a rough surface.

Transparency. Opake, or at most slightly translucent on the edges.

Streak. White.

Hardness. Semihard in the greatest degree.

Frangibility. Pretty easily frangible.

Specific gravity 2·510. Not particularly heavy. According to an analysis of this fossil performed by Dr. John, by a method which he will hereafter describe, its constituents are as follows:—

Silica	44·62
Alumina	37·36
Lime	2·75
Soda	8·00
Water	6·00
Oxide of iron	1·00
Loss	0·27
	100·00

There is no other mineral, among those already described, which possesses the characters and yields the same constituents as this, among which the proportions of soda and water are the most remarkable. On that account I have placed it in the system as a new peculiar species of mineral, belonging to the genus of siliceous stones.

* From the Gesellschaft Naturforschender freunde zu Berlin Magazin 1810, p. 78.

As fresh broken specimens of this mineral look as if they were spotted with coagulated blood, I have given it the name of lythrodes (from το λυθρον).

Some of the varieties of this mineral, where two of the plates that occasion the concealed foliated fracture are set perpendicularly upon each other, may be mistaken for a species of felspar; but a closer inspection, together with the consideration of its constituents, will undeceive us. When lythrodes exhibits small splendent particles, it contains labradore felspar mixed with it. The other substances occasionally mixed with this mineral are black hornblende, white analcime, and dark brown zircon.

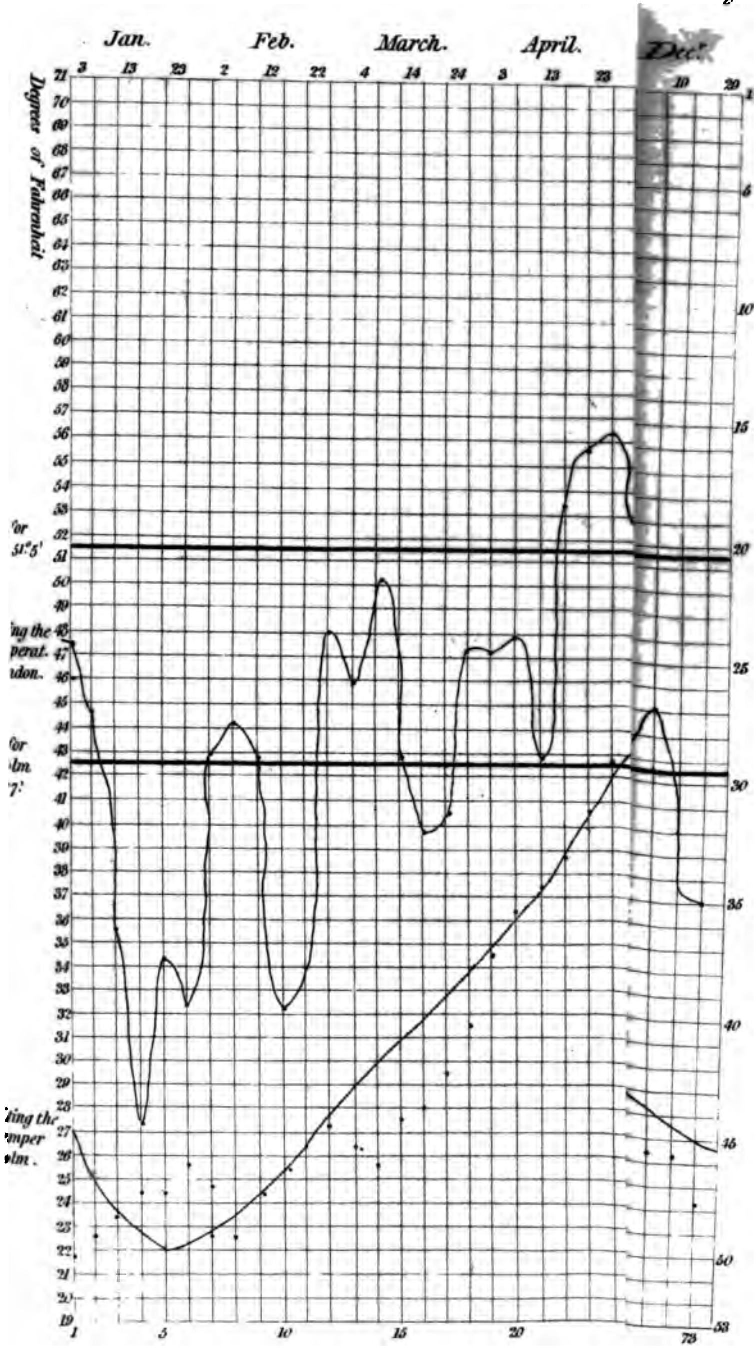
Article IV.

On the Difference of Temperature, during a period of 50 *years, at Stockholm, according to Observations made at the Observatory of the Academy of Sciences.* By J. Öfverbom.*

The process by means of which the sun's rays communicate heat to our earth will probably continue long unknown. Notwithstanding the attempts of Bouguer and Lambert, the doctrine that their greater or smaller intensity depends upon their angle of incidence, and upon the greater or smaller portion of the atmosphere through which they pass, seems still to prevail. Experience shows us that the sun produces the greater quantity of heat the more highly he is elevated; but at the same time that the heat is greater, a preceding day acquires the power of leaving an excess upon the following day. What confirms this proposition is, that the greatest heat does not happen at the summer solstice, but sometimes more than a month after it; just as the greatest heat of the day happens a little after two o'clock.

The changes in the winds and the thickness of the atmosphere, together with the variations of the barometer and hygrometer, and probably many other circumstances not yet understood, occasion the heat of one year to differ from that of another, even though the observations be made at the same time of the day, and be kept regularly during the whole year. But as the power of the sun ought to be the same every year, one may hope that by taking a great many years together their differences and anomalies will be sunk in each other, and the

* From Kongl. Vetenskaps Academiens nya Handlingar, tom. 29, p. 294, for 1808.



Comparative.

f the sun and the laws of temperature may be thence

method is, perhaps, the only possible one in a science its infancy, and which not readily subjecting itself to has proceeded from its very commencement by empi-

observatory of the academy the different heights of the neter have been generally marked three times a day, period of more than 50 years. As this period seems t to compensate for all anomalies, I have collected the temperatures of half a century of years, from 1758 both inclusive.

e found the medium temperature, from no fewer than bservations, to be 5¾°, or 5·765° above the freezing 2·377 Fahrenheit). From this medium every particular riated more or less, as will be seen by the following

Medium temperat.	Difference from the general medium.	Years.	Medium temperat.	Difference from the general medium.
+4·561°	−1·204	1784	+4·446°	−1·319
6·132	+0·367	85	4·628	−1·137
5·196	−0·571	86	4·421	−1·344
6·402	+0·637	87	5·757	−0·008
5·802	+0·037	88	4·693	−1·072
4·940	−0·825	89	7·004	+1·239
6·367	+0·602	1790	6·374	+0·609
5·851	+0·086	91	7·610	+1·845
6·577	+0·812	92	5·975	+0·210
5·227	−0·538	93	6·503	+0·738
5·152	−0·613	94	7·811	+2·046
5·402	−0·363	95	4·921	−0·844
5·656	−0·109	96	6·441	+0·676
4·672	−1·093	97	6·824	+1·059
5·116	−0·649	98	6·980	+1·215
7·419	+1·654	99	4·101	−1·664
5·226	−0·539	1800	4·970	−0·795
7·696	+1·931	01	5·881	+0·116
6·313	+0·548	02	5·531	−0·234
5·378	−0·387	03	4·712	−1·053
5·729	−0·036	04	4·747	−1·018
7·775	+2·010	05	4·062	−1·703
5·838	+0·073	06	5·411	−0·354
6·582	+0·817	1807	5·543	−0·235
4·868	−0·897			
7·032	+1·267	Mean	5·765	

The year may be divided into 73 penthemerons (or portions of five days each), with some little anomaly for the intercalary days. I have made this division to show the temperature of the year more equally. In the following table, the first number, — 5·596 (for example) is the mean of all the thermometer heights for the first five days of January, during all the 50 years, and of course the medium of 750 observations. The same remark applies to all the other numbers.

To convey a distinct idea of the ascending and descending progression of the medium observations, I have not only drawn up the following table, but likewise constructed the diagram (plate 2) *, where the dots denote the mean heights actually observed, and the curve line the probable hypothetic mean.

A glance of the eye shows how nearly they agree, as long as the thermometer is above the mean temperature of the year, but that the difference between them goes as far as 2 degrees (3·6 degrees Fahrenheit) during the three first months of winter.

Number of penthemerons.	Middle day of the penthemeron.	Medium of 50 years' observations.	Probable mean temper.	Difference.	Column 3d in degrees of Fahrenheit.
1	Jan. 3	—5·596°	—4·521°	—1·075	21·927*
2	8	5·098	4·898	—0·200	22·824
3	13	4·791	5·214	+0·423	23·376
4	18	4·215	5·456	+1·241	24·413
5	23	4·193	5·572	+1·371	24·453
6	28	3·401	5·421	+2·020	25·878
7	Feb. 2	3·942	5·165	+1·223	24·904
8	7	4·731	4·838	+0·107	23·484
9	12	4·690	4·451	—0·239	23·558
10	17	3·683	4·018	+0·331	25·403
11	22	2·995	3·532	+0·537	26·609
12	27	2·685	3·008	+0·323	27·167
13	March 4	3·229	2·446	—0·783	26·188
14	9	3·660	1·848	—1·812	25·410
15	14	2·272	1·216	—1·056	27·910
16	19	2·190	0·552	—1·638	28·060
17	24	1·168	+0·143	—1·311	29·898
18	29	0·281	0·867	—1·148	31·494
19	April 3	+1·273	1·620	—0·347	34·295
20	8	2·470	2·399	+0·071	36·446
21	13	3·009	3·205	—0·196	37·416

* The degrees of the thermometer in the original are those of Celsius. For the sake of the English reader they have been changed in the plate for those of Fahrenheit. The Editor has likewise inserted the mean temperature at London during 1810, by way of comparison with that of Stockholm.

Number of penthemerons.	Middle day of the penthemeron.	Medium of 50 years' observations.	Probable mean temper.	Difference.	Column 3d in degrees of Fahrenheit.
22	April 18	+3·776°	+4·037°	−0·261	38·797°
23	23	4·926	4·893	+0·033	40·867
24	28	5·773	5·775	−0·002	42·391
25	May 3	6·572	6·828	−0·256	43·830
26	8	7·502	7·849	−0·347	45·504
27	13	8·279	8·838	−0·559	46·902
28	18	10·136	9·793	+0·343	50·245
29	23	10·888	10·713	+0·175	51·598
30	28	11·446	11·597	−0·151	52·603
31	June 2	13·026	12·442	+0·548	55·447
32	7	13·978	13·248	+0·730	57·160
33	12	14·442	14·013	+0·429	57·996
34	17	15·237	14·733	+0·504	59·427
35	22	15·423	15·406	+0·017	59·761
36	27	16·089	16·028	+0·061	60·960
37	July 2	16·572	16·596	−0·024	61·830
38	7	17·272	17·102	+0·170	63·089
39	12	17·556	17·538	+0·028	63·619
40	17	17·839	17·892	−0·053	64·110
41	22	18·068	18·133	−0·069	64·515
42	27	18·180	18·103	+0·077	64·720
43	Aug. 1	17·856	17·838	+0·018	64·141
44	6	17·449	17·470	−0·021	63·408
45	11	17·211	17·020	+0·191	62·898
46	16	16·446	16·503	−0·057	61·603
47	21	16·197	15·926	+0·271	61·155
48	26	15·094	15·295	−0·201	59·169
49	31	14·326	14·614	−0·288	57·787
50	Sept. 5	13·734	13·886	−0·152	56·721
51	10	12·924	13·114	−0·190	55·263
52	15	11·993	12·301	−0·308	53·587
53	20	11·132	11·449	−0·317	52·038
54	25	10·459	10·559	−0·100	50·826
55	30	8·998	9·633	−0·635	48·196
56	Oct. 5	8·785	8·672	+0·113	47·613
57	10	7·513	7·678	−0·165	45·523
58	15	6·612	6·651	−0·039	43·901
59	20	5·604	5·618	−0·014	42·087
60	25	5·251	4·746	+0·505	41·452
61	30	4·177	3·894	+0·283	39·512
62	Nov. 4	3·787	3·067	+0·720	38·817
63	9	3·155	2·265	+0·890	37·679
64	14	1·872	1·490	+0·382	35·369
65	19	0·176	0·742	−0·566	32·317

Number of penthemerons.	Middle day of the penthemeron.	Medium of 50 years' observations.	Probable mean temper.	Difference.	Column 3d in degrees of Fahrenheit.
66	Nov. 24	−0.383°	+0·023°	−0·406	31·811°
67	29	0·611	−0·667	+0·056	30·900
68	Dec. 4	1·405	1·326	−0·079	29·471
69	9	1·602	1·952	+0·350	29·117
70	14	2·165	2·544	+0·379	28·103
71	19	3·155	3·100	−0·055	26·321
72	24	3·164	3·617	+0·453	26·305
73	29	4·334	4·092	−0·242	24·199
	Mean +	5·765			

Article V.

*Experiments on Niccolanum.** By W. Hisinger and C. A. Murray.

Mr. J. B. Richter published in the *Neues Allg. Journal der Chemie*, Vol. iv. page 392, his experiments on a new metallic body, which he discovered during his attempts to reduce the pure oxide of nickel to the metallic state, and which he called *Niccolanum, a new discovered metal, very similar in many respects to nickel.* It was chiefly distinguished from nickel in not being reducible by mere heat, in being more violently attacked by nitric acid, and in the different colours of the precipitates obtained from the solutions of it in acids. Through the goodness of Mr. Gehlen, we obtained various specimens of this new metal, soon after its discovery. One of these specimens was subjected to the following experiments:

(a) A regulus of niccolanum, which weighed 2243 milligrams, (34·54 grains Troy) was digested in pure dilute nitric acid. The action was moderate; but was increased when the acid became more concentrated. The colour of the solution was emerald green, exactly similar to that of nickel. The acid left undissolved a small yellowish brown mass, which was separated, and boiled for a long time in concentrated nitric acid, but was not sensibly dissolved. Muriatic acid was therefore employed, which gradually dissolved the whole of the mass, and assumed a greenish yellow colour. The solution being diluted with a great deal of water, was mixed with an excess of caustic ammonia, in a glass vessel, furnished with a stopper, but no precipitate fell. By gentle evaporation in a retort, a reddish brown oxide was thrown

* From Afhandlingar i Fysik, Kemi och Mineralogi. Utgifne af W. Hisinger och I. Berzelius, Vol. iii. p. 105.

down, which though mixed with an excess of warm caustic ammonia was not dissolved. The liquid was colourless, which proved that it contained in solution neither nickel, niccolanum, nor cobalt. It was filtered, and the sal ammoniac evaporated to dryness. During this evaporation more oxide was precipitated. A sufficient quantity of caustic potash was employed to decompose the whole of the sal ammoniac; the mixture was then dissolved in water, and a small portion of undissolved oxide remained behind. That these different precipitates consisted of the oxide of iron was ascertained by treating them before the blow-pipe with phosphate of ammonia, saltpetre, &c.; but the quantity at the highest estimation did not exceed two milligrams (·03 grain Troy).

(b) A current of sulphureted hydrogen gas was passed through the solution of niccolanum in nitric acid. For some time no change was perceptible, but after the interval of an hour it began to grow muddy, and yellowish flocks were separated, which fell to the bottom of the vessel. These being separated and dried had a blackish brown colour, but their quantity was so small that they could not be weighed. Before the blow-pipe they exhibited the properties of sulphuret of arsenic, and emitted unequivocally the smell of arsenic. Hence it follows, that niccolanum contains a small portion of arsenic; but it is quite free from the least trace of copper.

(c) The solution was exactly saturated with caustic potash. The precipitate which fell was, at the instant of precipitation, of a whitish green colour, but it gradually changed into a light greenish and bluish grey colour, or a mixture of blue and green oxide like what is obtained from a solution of cobalt alloyed with nickel. That portion of the precipitate which attached itself to the sides of the glass, and was not in contact with the liquid, had a light green colour. When diluted with water the precipitate appeared of a light mountain green. It was mixed with water, which was drawn off repeatedly by a syphon, till all the salt disappeared. As from the mixed colours of the precipitate, and from several other circumstances, we began to suspect that it was a mixture of the oxides of nickel and cobalt, we made choice of the method proposed by Thenard, in the *Allg. Jour. der Chemie*, Vol. iv. p. 284, to separate these metals from each other. For that purpose we prepared a solution of lime in oxymuriatic acid, and agitated the precipitate in this solution in a well stopped glass flask. It was immediately changed into an oxide, containing more oxygen, and having a black colour. This black oxide was freed from all muriate of lime by washing it with water till the liquor ceased to precipitate nitrate of silver. Caustic ammonia was added in excess: it assumed a deep blue colour without inclining to red or garnet (as Richter alleges) either when seen by the light of day or by a candle. The oxide, being allowed to remain for several days under the ammonia, diminished in quantity and became lighter coloured. The solution in ammonia, being drawn off by a syphon, was clear and pure blue, and was evaporated to dryness without undergoing the smallest change of colour. The residue, after evaporation, was a light green oxide, which when heated to redness became black, and weighed 427 milligrammes (6·58 grains). It dissolved before the blowpipe in phosphate of soda, and the bead had a blood-red colour while hot, but became honey yellow when cold, and when saltpetre was added gave a bluish coloured bead. In borax it dissolved with difficulty, and the colour of the bead was reddish brown. The solution of it in muriatic acid formed characters on paper of a green colour, scarcely perceptible while warm. All these properties show clearly that the oxide examined was pure oxide of nickel.

(d) The liquid remaining after the precipitation of niccolanum by caustic potash (c), and the water employed in washing the precipitated oxide, being boiled, assumed a green colour, and a small portion of green oxide precipitated, which was added to the oxide examined in the preceding paragraph. The liquid being concentrated to crystalization, and the mother lye mixed with nitrate of silver, it exhibited no trace of arsenic acid.

(e) The portion of oxide, which the caustic ammonia had left undissolved, was again digested in a fresh quantity of ammonia. The solution became bluish, but exhibited also a shade of red or amethyst, especially when examined by candle-light. The ammonia had therefore taken up a portion of cobalt, which, by its long continued action, it had brought down to a lower degree of oxydizement, and rendered soluble. By slow evaporation a greenish oxide of nickel was obtained, and the liquid assumed a bluish colour, but which at last was changed into a reddish. The precipitated oxide, being separated and examined before the blow-pipe with borax, exhibited evident marks of containing cobalt.

(f) As the oxide of cobalt, from the preceding experiments, was found to be soluble in ammonia, and again brought to a minimum of oxydisement, an attempt was made by a new solution in muriatic acid, precipitating by caustic potash, washing the precipitate, and agitating it with oxymuriate of lime, to render the cobalt again insoluble, till the whole of the nickel was separated from it: but the attempt did not succeed; for the solution in ammonia was violet coloured, and contained both oxides. The colour of the oxide changed, in a very short time, from black to grey.

(g) As this method of separating the two metals was unsuccessful, we had recourse to the method of Proust, by repeated ystalizations. The oxides dissolved in ammonia in paragraphs) and (f), and the portion which the ammonia would not issolve, were freed from ammonia by washing. Sulphuric acid was ien poured upon the mass, which dissolved the whole, except a w flocks which consisted of oxide of iron, and which weighed, hen heated to redness, 5 milligrammes (·077 grain Troy). The ear solution in sulphuric acid, which had an amethyst red olour, was mixed with caustic ammonia, and set aside in a oderate warmth, in order to evaporate slowly, and crystalize. here shot first deep green crystals of sulphate of ammonia and ckel: these being removed, the liquid remained rose red; ıd, finally, gave a confused mass of a dark rose red colour.

(h) The green and red salts thus obtained were dissolved parately in water, and decomposed by boiling them with ustic potash. The hydrates of nickel and cobalt thus obtained ere, the first mountain green, the second blackish green; but ing washed, and heated to redness, they became both blackish. he oxide of cobalt weighed 150 milligrammes (2·31 grains). gave before the blow-pipe with borax a dark blue glass. The balt metal was precipitated from the smalt by a polished copper ire. It dissolved in nitric acid by the assistance of sugar, and e solution was colourless. When the solution was neutralized, ıd mixed with benzoate of ammonia, it exhibited no trace of m. With muriatic acid it gave a sympathetic ink. It was, erefore, a very pure oxide of cobalt. The green salt gave, ter decomposition and beating to redness, a blackish oxide of ckel, which weighed 2055 milligrammes (31·65 grains). It ssessed the same properties as the oxide obtained in paraaph (c).

The portion of regulus of niccolanum examined, therefore, ich originally weighed 2243 milligrammes (34·54 gr.), had en decomposed into the following constituents:—

	M. Gr.	M. Gr.
Black oxide of nickel	2482	
Which, reckoning 0·20 for oxygen, gives of metallic nickel		1985·60
Black oxide of cobalt	150	
Which, reckoning 0·20 for oxygen, gives of metallic cobalt		120·00
Red oxide of iron	7	
Which, reckoning 0·30 for oxygen, gives of metallic iron		4·90
A trace of arsenic		
Loss		132·50
	2639	2243·

Conclusion.

From the preceding experiments we may conclude,

1. That niccolanum is not a peculiar new metal.

2. That it is a compound of nickel, $6\frac{1}{4}$ per cent. of cobalt, a little iron, with a trace of arsenic.

3. That the method proposed by Thenard to separate nickel from cobalt does not succeed,

4. That careful crystalization with sulphate of ammonia or potash is the best method of separating these two metals from each other.

Article VI.

Report of a Select Committee of the House of Commons on Transportation.

(As ordered to be printed July 10, 1812.)

(*Concluded from p.* 51.)

Though the religious feeling in the colony appears to have been weak, latterly the erection of places of worship, and the establishment of clergymen, have not been neglected. Churches have been built at Sydney and Paramatta, and in Hawkesbury the service was performed in houses appropriated to that purpose; and to each of these districts clergymen have been appointed, with a sufficient provision from government. In Governor Hunter's time, the attendance of the convicts was enforced at church. This compulsion appears to have been neglected during the government of Admiral Bligh; though during the hours of divine service all loitering was forbidden in the town of Sydney. No restraint is imposed on those professing a different religion, and Roman Catholic clergymen have been allowed to perform the rites of their church; registers of baptisms, marriages, and burials, are regularly kept, and many schools have been established. The Orphan Female School, supported by port duties and fines, has flourished almost from the first settlement of the colony, and a Male Orphan School, on a similar plan, has lately been established; several private schools are also open, and the education of youth appears by no means to be neglected, though the want of proper masters has been much felt at different periods.

Your committee have been thus particular in detailing the regulations, and the natural and commercial advantages of the settlement, because they strongly feel that its improvement in wealth, and the means of properly employing and reforming the convicts, are essential to the progress of each other; if the prosperity of the colony be checked by unwholesome restrictions, the exertions and industry of the convicts cannot be advantageously called into action during their servitude, and but little inducement will be held out to them to become settlers after their emancipation. They will now proceed to detail the manner in which the transportation of the convicts is conducted, and what are their government and treatment within the colony.

When the hulks are full up to their establishment, and the convicted offenders in the different counties are beginning to accumulate, a vessel is taken up for the purpose of conveying a part of them to New South Wales. A selection is in the first instance made of all the male convicts under the age of 50, who are sentenced to transportation for life and for 14 years; and the number is filled up with such from amongst those sentenced to transportation for 7 years, as are the most unruly in the hulks, or are convicted of the most atrocious crimes: with respect to female convicts, it has been customary to send, without any exception, all whose state of health will admit of it, and whose age does not exceed 45 years.

The Irish convicts have generally been sent with less selection than those from England; and this has arisen from the want of hulks, and other means of confining and employing them, which are here often substituted for transportation: but as this is a subject now under arrangement, and occupying much of the attention of the Irish Government, your committee forbear making any observations upon it.

The evidence of Mr. M'Leay distinctly and satisfactorily explains the manner in which they are transported. An order is received from the Treasury at the Transport Office, to take up vessels for New South Wales. They are advertised for, and the lowest tender accepted. Clothing and provisions for the support of the convicts during the voyage, and nine months afterwards, are sent from the Victualling Office, and medicines are furnished from Apothecaries Hall. The owner of the vessel provides a surgeon, who undergoes an examination at Surgeon's Hall and the Transport Office. He is instructed to keep a diary not only of the illness on board, but of the number of convicts admitted on deck; of the scraping the decks, cleaning the births, and general treatment of the transports. The sick are to be visited twice a day, the healthy once. He is ordered to take the greatest precaution against infection, and to fumigate the clothes of those taken to the hospital. He has not only power to use medicines, but also the stores, if any sick be in want of greater nourishment. He is further instructed to transmit to the Secretary of State any observations which may occur to him productive of improvement in the mode of treatment, and he is paid a gratuity of 10*s.* 6*d.* for every convict landed in New South Wales. The instructions to the master are equally satisfactory. He is to be particularly cautious to receive no diseased person on board during the voyage; a proportion of the prisoners is daily to be admitted upon deck, and the births of all cleaned and aired; and these things are to be noted in the log-book, which is afterwards submitted to the Governor of New South Wales; and if the conduct of the master appears to have been satisfactory, he receives a gratuity of 50*l.* If the contrary should turn out to be the case, a power of mulcting him is given by the contract, and he becomes liable to a prosecution. The ration of provision is fixed, and appears to be amply sufficient for the support of the men; about 200 men or women are generally embarked on board one ship, with a guard of 30 men and an officer. Such are the present regulations for the voyage; and however bad the treatment of the convicts on board the vessels may formerly have been, the present system appears to your committee to be unobjectionable. The witnesses speak of it in terms of high commendation, particularly two of those who have been sent out as convicts. Governor Macquarie, in his last dispatches, mentions the good treatment of the prisoners on board the two transports last sent out; and a still stronger proof of the improvement in the mode of conveyance is, that from the year 1795 to 1801, of 3,833 convicts embarked 385 died on board the transports, being nearly 1 in 10; but since 1801, of 2,398 embarked, 52 only have died on the passage, being 1 in 46. The only further observation your committee have to make on this part of the subject is, one of regret that no arrangement whatever is made for the performance of Divine Service during this six months' voyage; that this, which is the heaviest part of their punishment, is also the least likely to produce reformation. With the dispatches from Government a list of the convicts is generally sent, but this list has for the most part been very deficient in particularising the offences of which they have been convicted; and in distributing them upon their arrival, the Governor has no clue to guide him in giving to them more or less advantageous situations, according to the nature of their crimes and characters: this is a neglect easy and at the same time most necessary to be corrected. Upon the arrival of a transport, general orders are issued for returns of the number of men wanted, with the land held in cultivation by each settler. The trade, age, character, and capacity of the convicts are, as far as possible, investigated; the artificers are in general reserved for the service of Government, and as many of the others as may be wanted. Persons who have been in a higher situation of life have tickets of leave given to them, by which they have liberty to provide for themselves, and are exempt from all compulsory labour; similar tickets are given to men unused to active employment, as goldsmiths and others; the remainder are distributed amongst the settlers as servants and labourers. The convicts in the service of Government are divided into gangs,—every gang has an overseer, and every two or three gangs a superintendent; these are frequently chosen from amongst those convicts who best conduct themselves. They work from six in the morning till three in the afternoon, and the remainder of the day is allowed them, to be spent either in amusement or profitable labour for themselves. They are clothed, fed, and for the most part lodged by Government; and though in the early periods of the colony, inconvenience and distress may have arisen from the irregularity of supply from this country, latterly the food and clothing have been good, and, generally speaking, in sufficient abundance. Should the convicts misconduct themselves at their work, the superintendents have no power of inflicting punishment, but are for that purpose obliged to take them before a magistrate; the sitting magistrate of the week at Sydney may order a punishment of 25 lashes; a regular bench, which consists, at least, of three, may order as many as 300; and in the distant parts of the colony, a single magistrate has the same power with the bench at Sydney; but a heavy punishment is not executed without the previous approbation of the Governor. Another mode of correction, and that which your committee would recommend to be preferred, in as many cases as possible, is to sentence the culprit to work for a certain number of days in the gaol gang: he is here obliged to labour at some public work in irons, from six in the morning to six at night, and no hours are allowed to him for profit or amusement. The convicts distributed amongst the settlers, are clothed, supported, and lodged by them; they work either by the task or for the same number of hours as the Government convicts; and when their set labour is finished, are allowed to work on their own account. The master has no power over them of corporal punishment, and this can only be inflicted by the interference of a magistrate; even if the master be a magistrate himself, he can order no punishment to his own servant, but must have recourse to another magistrate. If the servant feels himself ill used by his master, he has power of complaining to a magistrate, who will, if the complaint be well founded, deprive the master of his servant. It is so much the interest of the settlers to keep their servants in good health, and to attend to their conduct, that your committee have heard no evidence but in commendation of their treatment, and of its effects upon their morals and comfort. Indeed it is most manifest that where two or three convicts are domiciled in a family, removed from their former companions, and forced into habits of industry and regularity, the chance of reformation must be infinitely greater than when they are worked in gangs, living with each other amidst all the inducements to vice which such a town as Sydney must afford to them; and such, by all the evidence, appears to be the effect of this system of distributing them amongst the settlers. Nor is it to be lost sight of, that in the service of settlers they are likely to acquire some knowledge of farming; and that if, from convicts, they became well-behaved and industrious servants, a farther possibility is opened to them of becoming prosperous and respectable settlers. On these grounds your committee recommend as much as possible their distribution as servants and labourers to individuals; and they have observed with much satisfaction, that such appears to be the system pursued at present by Governor Macquarie; nor will such an arrangement materially increase the expense to Government, or impede the progress of its works. It is to be found in the evidence of Mr. Commissary Palmer, that the expense of each convict in the service of Government was about 40*l.* a year, and that a free labourer at Sydney could be hired for 70*l.* but that he would do nearly twice as much work. Mr. Campbell states the annual expense of a convict at 30*l.*, but in the other point he agrees with Mr. Palmer. Some of the benefits of this system must be lost where too many convicts are given to one master, and in some instances 40 have been put under the control of a single settler; but from the extent of some of the farms, such a distribution appears to be unavoidable. In the distribution of female convicts great abuses have formerly prevailed; they were indiscriminately given to such of the inhabitants as demanded them, and were in general received rather as prostitutes than as servants; and so far from being induced to reform themselves, the disgraceful manner in which they were disposed of operated as an encouragement to general depravity of manners. Upon the arrival of Governor Bligh two-thirds of the children annually born within the colony were illegitimate. Marriages have latterly become more frequent, consequently prostitution is stated to have been less prevalent; and Governor Macquarie is directing his endeavours, under orders from the Government here, "to keep the female convicts separate till they can properly be distributed among the inhabitants, in such manner as they may best derive the advantages of industry and good character." He further states in his dispatch, dated April 30, 1810, that the situation of the colony requires that as many male convicts as possible should be sent thither, the prosperity of the country depending on their numbers; whilst, on the contrary, female convicts are as great a drawback as the others are beneficial. To this observation your committee feel they cannot accede: they are aware that the women sent out are of the most abandoned description, and that in many instances they are likely to whet and to encourage the vices of the men, whilst but a small proportion will make any step towards reformation; but yet, with all their vices, such women as these were the mothers of a great part of the inhabitants now existing in the colony, and from this stock only can a reasonable hope be held out of rapid increase to the population; upon which increase, here, as in all infant colonies, its growing prosperity in great measure depends. Let it be remembered too, how much misery and vice are likely to prevail in a society in which the women bear no proportion to the men; in the colony at present, the number of men compared to that of women, is as two to one; to this, in great measure, the prevalence of prostitution is reasonably to be attributed; but increase that proportion, and the temptation to abandoned vices will also be increased, and the hopes of establishing feelings of decency and morality amongst the lower classes, will be still farther removed.

The supply of women to the colony must, however, be materially diminished by the proposed system of employing convicts in penitentiary houses; and your committee think this an additional reason for affording increased facilities to the wives of male convicts, who may wish to accompany or follow their husbands to New South Wales. This permission is now seldom granted, and that only to the wives of men transported for life or for 14 years. It is however the most eligible way of providing the colony with women, and one which may with very great advantage be much extended.

At the expiration of the time to which the convicts have been sentenced, their freedom is at once obtained, and they are at liberty either to return to this country, or to settle in New South Wales; should the latter be their choice, a grant is made to the unmarried, of 40 acres of land, and to the married, of something more for the wife and each child: tools and stock (which they are not allowed to alienate,) are also given to them, and for 18 months they are victualled from the Government stores. In this manner, they have an opportunity of establishing themselves in independence, and by proper conduct to regain a respectable place in society; and such instances, your committee are glad to learn, are not unfrequent. They also see with satisfaction, that Governor Macquarie adopts it as a principle, "that long-tried good conduct should lead a man back to that rank in society which he had forfeited, and do away, in as far as the case will admit, all retrospect of former bad conduct:" this appears to him to be the greatest "inducement that can be held out towards the reformation of the manners of the inhabitants." In these principles your committee cordially concur, and are the more anxious to express their opinion, as, under a former Governor, transports, whatever their conduct might be, were in no instance permitted to hold places of trust and confidence, or even to come to the Government House; those advantages being, in his opinion, not to be expected until after generations.

The same advantages as are allowed to convicts having served their time, are given to those who have been pardoned or emancipated by the Governor; and your committee do not wish to dismiss the subject, without making some observations upon the power possessed by him of granting to convicts either the entire or partial remission of their sentence, or tickets of leave, by which they are altogether relieved from its severity. They do not see any necessity for the Governor's possessing a power to grant these absolute or conditional pardons; it is a power liable to great abuse, and which appears to have been at times very much abused. It is in evidence, that in some years 150 pardons have been granted; that pardons have been granted to convicts immediately upon their arrival, without reference to their characters or merits; and it appears rather to have at times been made an instrument to gain popularity, than the means of rewarding exemplary conduct by a well-deserved extension of his Majesty's mercy. Your committee therefore suggest, that no pardon whatever, real or conditional, be granted but through the Secretary of State. This may create a delay perhaps of a year, in obtaining the pardon of any convict, but that inconvenience will not be great, for by granting to him a ticket of leave, the convict will in the mean time be entirely relieved from the pressure of his sentence. Upon the subject of tickets of leave, your committee feel that the power of granting them ought to remain in full force with the Governor; but it is a power which they would wish to see sparingly and cautiously made use of; and with this view, they recommend that an annual return be made to the Secretary of State's office, of the number of tickets of leave issued in the year, with a statement of the grounds upon which each was granted.

No difficulty appears to exist amongst the major part of the men who do not wish to remain in the colony, of finding means to return to this country. All but the aged and infirm easily find employment on board the ships visiting New South Wales, and are allowed to work their passage home; but such facility is not afforded to the women: they have no possible method of leaving the colony but by prostituting themselves on board the ships whose masters may chuse to receive them. They who are sent to New South Wales, that their former habits may be relinquished, cannot obtain a return to this country, but by relapsing into that mode of life, which with many has been the first cause of all their crimes and misfortunes. To those who shrink from these means, or are unable even thus to obtain a passage for themselves, transportation for seven years is converted into a banishment for life, and the just and humane provisions of the law, by which different periods of transportation are apportioned to different degrees of crime, are rendered entirely null: to see this defect in the punishment remedied, is the anxious wish of your committee; and they trust that means may be devised to facilitate the return of such women as have passed their time of servitude, and are unwilling to remain in the colony, either by affording them a sufficient sum of money, or by some stipulation in their favour with the masters of vessels touching at the settlement.

It will be seen by the accounts laid before your committee, that the expenses of the colony are considerable. The bills drawn in the year 1810 amounted to 72,600, being a great increase upon any preceding year, and the expenditure of the year 1811 promised to be still greater: in addition to these, a great annual expenditure is incurred in the transmission of stores and merchandize, and in the freight of transports. Your committee trust that when the buildings absolutely necessary for the public service shall be completed, as the commerce of the colony shall prosper, the duties become more productive, and, from agricultural improvement, the supply of stores to its present amount shall be discontinued, that this expense will be materially diminished; and it is their opinion, that it might even now be considerably reduced by the removal of part of the military force in the colony, which appears to them to be unnecessarily large. The whole population does not amount to 11,000, and of these 1,100 are soldiers.

Such is the view taken by your committee of the colony of New South Wales; and it is, in their opinion, in a train entirely to answer the ends proposed by its establishment. It appears latterly to have attracted a greater share of the attention of Government than it did for many years after its foundation; and when the several beneficial orders lately sent out from this country, and the liberal views of the present Governor, shall have had time to operate, the best effects are to be expected. The permission of distillation within the colony, and the reform of the Courts of Justice, are two measures which your committee, above all others, recommend as most necessary to stimulate agricultural industry, and to give the inhabitants that confidence and legal security which can alone render them contented with the Government under which they are placed.

10*th July*, 1812.

TABLE I.

General Statement of the Inhabitants in his Majesty's Settlement on *Eastern Coast of New South Wales; with an Estimate of the* Remaim *Provisions in the Public Stores*, 1st *March*, 1810.

	Civil Department victualled			Military Department victualled.			Free Persons victualled.				Prisoners victualled from the Public Stores.			Number of different Rations.				Total Number of Persons victualled from
	Men.	Women.	Children.	Men.	Women.	Children.	Men.	Women.	Children.	Orphans.	Men.	Women.	Children.	At Full.	At Two Thirds.	At Half.	At One Quarter.	
At Sydney	22	1	3	1281	203	369	141	121	20	11	792	63	87	2245	428	356	83	311
At Paramatta..	8	-	-	85	14	40	32	47	79	3	243	65	12	371	126	108	23	6
At Hawkesbury	6	-	-	25	2	5	134	15	94	-	51	7	48	216	24	123	24	3
At Newcasle ..	1	-	-	25	-	-	-	-	5	-	46	16	7	72	16	7	5	1
General Total..	37	1	3	1416	219	414	307	183	198	14	1132	151	154	2904	594	594	135	4

Ration under Issue:—Seven pounds of Beef, or four pounds of Pork.
Eleven and a half pounds of Wheat.
Six ounces of Sugar; or, in lieu thereof, one pound of Wheat.

TABLE I. *continued.*

	People not victualled from the Public Stores.			Settlers not victualled from the Public Stores.			Total Number of Souls in the Settlement.	Week's Provisions in the Public Stores.			
	Men.	Women.	Children.	Men.	Women.	Total Number of Settlers not victualled.		Salted Beef.	Salted Pork.	Wheat and Rice as Flour.	Sugar.
At Sydney	986	939	1012	98	9	107	6156	-	-	-	-
At Paramatta	381	298	306	187	7	194	1807	-	-	-	-
At Hawkesbury	539	407	620	430	6	436	2389	-	-	-	-
At Newcastle	-	-	-	-	-	-	100	-	-	-	-
General Total	1906	1644	1938	715	22	737	10,452	-	-	-	-

*

Sydney, April 30, 1810.

(Signed) L. MACQUARIE,
Gov. in Chief, N. S. W

* These blanks are not filled up in the printed R? f.

TABLE II.

General Statement of the Inhabitants in his Majesty's Settlements at Norfolk Island, Port Dalrymple and Hobart Town, Van Diemen's Land, as accounted for by the respective Returns transmitted to Head Quarters.

	Governor and Commander in Chief.	Lieutenant Governor.	CIVIL DEPARTMENT VICTUALLED. Deputy Judge Advocate.	Commissary.	Principal Surgeon.	Deputy Provost Marshal.	Secretary to the Governor.	Clergymen.	Assistant Surgeons.	Surveyor of Lands.	Deputy Surveyor of Lands.	Deputy Commissaries.	Beach Master.	Overseers.	Superintendents and Storekeepers.	Women of the Civil Department.	Children above Two Years of Age.	Children under Two Years of Age.	Total of the Civil Department victualled.
At Norfolk Island	-	-	-	-	-	1	-	-	1	-	-	-	1	-	3	-	-	-	6
Port Dalrymple..	-	1	-	-	-	-	-	-	1	-	1	1	-	-	2	4	2	-	12
Hobart Town	-	1	1	-	1	-	-	1	1	-	1	1	-	3	5	4	1	1	21
General Total	-	2	1	-	1	1	-	1	3	-	2	2	1	3	10	8	3	1	39

	MILITARY DEPARTMENT VICTUALLED. Colonel.	Lieutenant Colonel.	Major.	Captains.	Lieutenants.	Ensigns.	Adjutant Paymaster and Quartermaster.	Surgeons.	Assistant Surgeons.	Serjeants and Corporals.	Drummers and Fifers.	Privates.	Women.	Children above Ten Years.	Children above Two Years.	Children under Two Years.	Total of the Military Department victualled.
At Norfolk Island	-	-	-	-	1	-	-	-	-	4	2	28	7	-	5	-	47
Port Dalrymple..	-	-	-	1	1	1	-	-	-	8	2	48	17	-	39	-	117
Hobart Town	-	-	-	-	2	-	-	-	-	6	2	37	12	-	5	14	78
General Total	-	-	-	1	4	1	-	-	-	18	6	113	36	-	49	14	249

Date of the Returns:—Norfolk Island, March 31, 1810.—Port Dalrymple, Feb. 24, 1810.—Hobart Town, Jan, 31, 1810.

TABLE II. *continued.*

	Free Persons victualled.							Prisoners victualled from the Public Stores.						Number of different Rations.						People not victualled from the Public Stores				Settlers not victualled from the Public Stores.				Week's Provisions in the Public Stores.		
	Men.	Women.	Children above Ten Years of Age.	Children above Two Years of Age.	Children under Two Years.	Orphans.	Total Number of Free Persons victualled.	Men.	Women.	Children above Ten Years.	Children above Two.	Children under Two Years.	Total Number of Prisoners victualled.	At Full.	At Two Thirds.	At Half.	At One Quarter.	Total Number of full Rations.	Total Number of Persons victualled from the Stores.	Men.	Women.	Children.	Total Number.	Men.	Women.	Total Number of Settlers.	Total Number of Souls in the Settlement.	Beef and Pork.	Wheat, Maize, and Rice, and Sugar as Wheat.	Sugar.
At Norfolk Island	61	18	-	16	-	3	98	25	1	-	-	-	26	130	26	21	-	159	177	-	-	-	-	-	-	-	177	$9\frac{1}{4}$	36	-
Port Dalrymple..	21	10	-	7	-	-	38	55	11	-	4	-	70	143	42	52	-	197	237	10	12	-	22	-	-	-	259	$9\frac{1}{2}$	39	27
Hobart Town	273	151	92	74	70	37	697	166	12	-	-	-	178	538	272	79	85	782	974	48	40	-	88	-	-	-	1062	$13\frac{1}{4}$	$7\frac{1}{2}$	24
General Total	355	179	92	97	70	40	833	246	24	-	4	-	274	811	340	152	85	1138	1388	58	52	-	110	-	-	-	1498	-	-	-

Sydney, New South Wales, April 30, 1810.

(Signed) L. MACQUARIE,
Gov. in Chief, N. S. Wales.

Article VII.

On the Cause of the Changes of Colour produced by Heat, on the Surface of Steel. By Sir H. Davy, LL.D. F.R.S.

(To Dr. Thomson.)

Dear Sir, Berkeley-square, Jan. 13, 1813.

In the last edition of your elaborate and learned System of Chemistry, vol. i. p. 224, you have stated that the changes of colour produced by heat on the surface of polished steel takes place under oil. In my Elements of Chemical Philosophy, page 390, I have said that these changes occur when the metal is plunged beneath the surface of mercury, and we both conclude that the effect probably does not depend upon the oxidisement of the metal.

I was led to doubt of the perfect correctness of our statements, and the justness of our conclusions, by a letter from Mr. Stoddart, who has made many accurate experiments on the tempering of steel; and that gentleman sent me two pieces of steel which had been heated to the same degree, one in the atmosphere and the other under the surface of pure mercury, where it had been suffered to cool; the first was blue, the second had suffered no change of colour; and both seemed to possess the same degree of hardness.

As I had formerly made but one experiment on this subject, and as the mercury I used was impure and not cleaned with any particular care, it appeared most likely that I had been deceived by some metallic oxides, or saline matter adhering to the mercury; and I invited Mr. Stoddart to assist in some new trials on the subject.

A piece of polished steel was introduced into a retort, which was exhausted and filled with hydrogene gas, and this hydrogene gas was deprived of oxygen, a small quantity of which might have entered with common air in the stop-cock, by melting phosphorus in it; the retort was then gradually heated. Where it was in contact with the steel, a slight tint of yellow was soon observed on the surface of the metal, but it did not increase as it would have done in the atmosphere during the increase of temperature.

A piece of polished steel was plunged in very pure olive oil, which had been previously heated to deprive it of air; the temperature of the oil was increased until it began to boil, but no change of colour took place on the surface of the steel.

I had little doubt that the slight change of colour produced on the metal in the hydrogene gas was owing to some aqueous vapour in the gas, or to some action of the phosphorus, and I have since proved the truth of this conjecture.

By heating polished steel in pure azote, deprived of aqueous vapour by sticks of potash over mercury, I found that no change of colour took place.

It appears evident, then, that the changes of colour produced during the tempering of the steel are owing to the formation and increase of a plate of oxide, and that they are mere indications of, and not connected with, that change in the arrangement of the particles of the steel which produce the diminution of its hardness.

If you should not deem this statement of too little importance for publication, you will oblige me by inserting it in your Journal. I am, dear Sir,

Very sincerely yours,

Humphry Davy.

Article VIII.

Observations on Mr. Klaproth's Analysis of the Water of the Dead Sea. By Alex. Marcet, M.D. F.R.S. one of the Physicians to Guy's Hospital.

(To Dr. Thomson.)

Sir,

In the first number of your "Annals of Philosophy," you have published an analysis of the water of the Dead Sea, by Mr. Klaproth, the results of which appear to differ so much from those which I laid before the Royal Society in 1807, and which were published in the Philosophical Transactions for that year, that I think it necessary to offer a few observations in reply to those which Mr. Klaproth has made upon our respective analyses.

I shall not trouble your readers with a repetition of the particulars of my analysis, which they may find detailed at full length in the volume of the Transactions above-mentioned; but as this experienced chemist has remarked, that the difference between his results and mine was probably owing to what he calls "the complicated processes and calculations" which I followed, and as his paper does not convey the smallest idea of the processes which he thus generally reproves, I am desirous of stating, in a few words, the leading points of the method I employed.

My first object was to ascertain, with all the care and accuracy of which I was capable, the composition of the principal salts concerned in this analysis; namely, the muriates of lime, of magnesia, of soda, and of silver;* these appearing to

* Some of these analyses of salts, the object of which was of a general import, I have had the satisfaction of seeing confirmed by the researches of Messrs.

me indispensable data, whatever mode of proceeding I might wish to adopt. I then tried various methods upon artificial solutions resembling the Dead Sea, the contents of which being previously known to me, I was enabled to ascertain, and compare with precision, the degree of confidence which these methods deserved. Having, at last, found one of them which yielded very accurate results, I proceeded to apply it to the water of the Dead Sea.

This method simply consisted in precipitating the two earths,* magnesia and lime, by appropriate re-agents, and calculating, from the precipitates obtained, the quantity of muriates to which these earths respectively belonged. The muriate of soda was first only inferred, from the quantity of muriatic acid found in the water; but, in another instance, it was actually obtained, after the separation of the earths, in the form of crystals; and the degree of coincidence in the results was such as can scarcely be exceeded in chemical analysis.

The state in which the weight of these salts was estimated, in summing up the contents of the Dead Sea, was that of perfect desiccation, such as can only be obtained by a red heat; and in the case of the muriate of magnesia (which cannot be actually heated to redness without undergoing decomposition), the necessary allowance for moisture was easily and accurately inferred from the known composition of this salt.

The method used by Mr. Klaproth (which, for the sake of comparison, it is necessary that I should here also state in a few words,) consisted in treating the residuum of the Dead Sea water, evaporated in a sand-bath, with alcohol, in order to separate the two earthy salts, which are soluble in this menstruum, from the muriate of soda, which is but sparingly dissolved by alcohol.† He then redissolved the earthy salts in

Davy, Berzelius, Gay-Lussac, and, in general, by those chemists who have turned their attention to the exact proportions in which bodies combine.

* The solid contents of the Dead Sea consist of muriate of magnesia, muriate of lime, and muriate of soda, with a vestige of sulphate of lime.

† That alcohol is capable of dissolving a certain proportion of muriate of soda (especially when there are deliquescent salts present which contain a great deal of water), Mr. K. seems to have been perfectly aware; for he treated the mass a second time with a smaller quantity of alcohol, in order to remove that source of error. As, however, he does not mention either the specific gravity, or the proportion of alcohol used in this second operation, it is natural to suppose that the same error would again occur, in some degree, in the second process. Having thought it worth while, in company with two friends, to try a direct experiment on the subject, about 100 grains of pure alcohol, of the specific gravity of 814, were mixed and agitated with a mixture of 5 grs. of muriate of soda, and 10 grs. of muriate of lime, in crystals. The residue left undissolved by the alcohol weighed only 4·5 grs.; and yet this residue was found to contain a notable quantity of lime; showing that the alcohol had dissolved some of the muriate of soda, without having taken up the whole of the muriate of lime; a circumstance which must, of course, have materially affected the results obtained by Mr. Klaproth's method.

water, precipitated the earths by carbonated alkali, converted them into sulphates, separated the sulphate of magnesia from the sulphate of lime by means of the greater solubility of the former, reprecipitated the magnesia in order to recombine it with muriatic acid, and from the quantity of this regenerated muriate of magnesia inferred that of the muriate of lime. The mass insoluble in alcohol was, of course, assumed to be muriate of soda. The salts in these various processes are stated to have been *evaporated to dryness*, *dried*, or in one instance *well dried*; but the mode or degree of desiccation is not mentioned.

I leave it to you, Sir, and to your readers, to decide which of these methods appears the most simple, the most direct, and the most likely to be accurate; but there is one circumstance in Mr. K.'s animadversions on my analysis which is too singular to be allowed to pass unnoticed. Indeed, it will at once show you that he had either only seen some erroneous abstract of my paper, or that he had totally overlooked some of its principal contents. After relating that I found 24·6 parts of saline matter in 100 of the water, he adds, "This estimate does not, however, accord with Dr. M.'s original statement, that 20 grs. of water leave a residuum of 7·7 grs. of dried salt. To make them agree, 100 grs. must have furnished 38·5 grs. of salt." Now the fact is, that I state distinctly, in my paper (p. 306) that the residue in question "dried at 180° weighed, whilst still warm, 8·2 grs.; and that the same saline mass being afterwards exposed in a sand-bath to the temperature of 212°, was reduced to 7·7 grs." From which I conclude, in the next paragraph, that "100 parts of the Dead Sea water yield 41 of salts dried at 180°, and 38·5 dried at 212°;" which is the exact conclusion the supposed want of which induced Mr. Klaproth to re-examine the water of the Dead Sea! As to the reduced proportion of 24·6 parts of salts in 100 of the water, which is stated as my ultimate result, it is, of course, fully and distinctly explained in the course of the paper (see p. 311), that this proportion applies to the case of perfect desiccation.

It is clear, therefore, that if Mr. Klaproth had read the whole paper, he would have seen that our results, as to the sum total of the salts contained in the water of the Dead Sea, far from being incompatible, may agree perfectly, since he found in 100 parts of water 42·5 parts of salts (dried at a temperature which he does not specify),* whilst my specimen of water yielded 41 parts dried at 180°.

* The account says, "dried upon a sand-bath till they no longer lost any weight," a latitude which may extend from the heat of the body, or even less, to that of incipient ignition. Mr. K. found the specific gravity of the water somewhat higher than I did; and he justly observes, that various specimens may vary a little in this respect; but from the circumstance of his specimen of water

With regard to the proportions which the several salts of the Dead Sea bear to each other, and the considerable differences which our conclusions exhibit in that respect, it is a subject upon which those who may have leisure to peruse and compare the two papers can form an opinion. I confess that the great care I bestowed on my analysis gave me some confidence in the results;* but the question of accuracy, as to the minute proportions of the salts of the Dead Sea, is of too little consequence to science to require any further discussion. It is only the analytical *methods* which I have thought it right to vindicate; and you will no doubt join me in strongly recommending to analysts the most scrupulous attention to the desiccation of salts. It is evidently to the constant interference of water that most of the apparent inconsistencies which have impeded the progress of analytical chemistry are to be ascribed; and it is only by the greatest care, in removing that source of confusion, that the proportions in which bodies unite can be well ascertained, and that chemical analysis can be made to keep pace with the late refined views of chemical combination.

I have the honour to be, &c.

Alex. Marcet.

Russel-square, Jan. 15, 1813.

Article IX.

Exposition of the Facts hitherto collected concerning the Effects of Vaccination, and Examination of the Objections made at different Times against the Practice. Read to the Class of Physical and Mathematical Sciences of the French Institute, by M. M. Berthollet, Percy, and Hallé, August 17, 1812.†

A report was read to the Institute in 1803 on this subject; and a memoir on the same subject, made at Lucca in 1806, was printed in their eighth volume. Now, after twelve years of experiments, repeated not only all over Europe, but in every part of the civilized world, we present the results deduced from the comparison of a multitude of facts, often inconsistent with

having begun to deposit crystals, I should suppose that it had undergone some accidental evaporation. The water which I examined, and which had been brought from the spot by *Mr. Gordon, of Clunie,* (and not *Messrs. Gordon and Clunie,* as Mr. K. has hastily stated,) was perfectly free from crystals.

* The advantage, also, of having had Mr. Tennant's occasional assistance in forming the plan, and conducting the various processes of this analysis, increased my confidence; and this circumstance, which Mr. Klaproth has noticed, should, I think, have induced him to examine the subject with a little more attention than he seems to have done.

† This important paper is translated, and in some places abridged, from the *Moniteur.*

each other, observed in all climates, and in all possible circumstances.

But notwithstanding the present general consent of physicians, governments, and the public at large, of the importance and advantages of vaccination, some voices have been raised against it. Whenever these objections have been made by honest and well-informed persons, not influenced by any personal interest, they are justly entitled to attention. Whatever may be our own opinion respecting this question, we are far from blaming those who think differently from ourselves. A spirit of opposition and independence is a valuable quality in the sciences of observation, when it happens to be united to information and talents, and when it is influenced, even if it goes astray, only by the love of truth, and the fear of yielding to precipitate enthusiasm. On that account, in the statement which we are about to lay before the Class, we shall draw our arrangement from the objections which have been made against vaccination by men of information.

1. The sensible effects of vaccination have been compared with those produced by inoculation for the small-pox. As the latter, after a fever more or less violent, terminates by an eruption of small-pox, it has been concluded that vaccination, as it occasions nothing similar, produces only an incomplete revolution in the body, and therefore must be less advantageous, and may even leave an injurious ferment which the local cow-pox has not been able to remove.

This first objection is purely theoretic; but those which follow are supported by facts conceived to be favourable to them.

2. When vaccination was first introduced into different countries, eruptions made their appearance upon different individuals. Severe, and in some cases fatal, diseases have accompanied these eruptions. It has been concluded that the virus introduced by vaccination was of such a nature as to produce these eruptions; but as they frequently do not appear, and are always irregular and incomplete, the virus in these cases, instead of being thrown out by the skin, according to the intention of nature, remains in the body, becomes the cause of different accidents, and may even produce alterations hurtful to the constitution.

3. Some accidents, and even diseases which have made their appearance, while the patient was under the effects of vaccination, have been ascribed to it, and have led to the opinion that the virus of the cow-pox was of a nature to produce dangerous and fatal diseases.

4. Sometimes after a cow-pox inoculation has happily terminated, but at no great interval of time, diseases have made their appearance, which have been ascribed to the influence of vacci-

mation. Hence it has been concluded, that even after apparent success, vaccination may be the source of chronical diseases more or less severe, and that it may leave the seeds of them in the body.

5. Finally, from comparing some facts in which the inoculation for the small-pox has been the epoch of a fortunate revolution in the health of some individuals, with the inconveniences which have been considered as the consequences of the cow-pox, some persons have thought, that even allowing both to be equally efficacious as preservatives against the small-pox, yet the small-pox inoculation has the advantage of often proving an efficacious remedy for several disorders over which the cow-pox has no influence.

Such are the strongest objections which have been made against vaccination. The other objections, being of less moment, will be considered more briefly. The first objection, to which we shall turn our attention in the first place is, in our opinion, the most feeble, reposing entirely upon a pathological theory. It may be comprehended under the following question.

FIRST QUESTION.

Do the fever and the general eruption which follow the inoculation for the small-pox, but do not appear after vaccination, constitute a necessary purification of the system, the want of which may lead to dangerous consequences?

The theory which admits, in a great number of acute and even of chronic diseases, a movement destined to produce evacuations more or less considerable, and by that means to throw out of the body a foreign matter which has given birth to the disease—this theory has been contrived in order to explain certain phenomena, which appear in succession during the course of some acute diseases, and the regular order in which these phenomena succeed each other, and terminate in the cure of the disease. The progress of several maladies is well adapted to this theory, nor can it be denied that the phenomena of the small-pox, whether natural or from inoculation, accord very easily with the principles upon which this theory has been built.

A quantity of the pus of small-pox, scarcely perceptible to the eye, introduced under the epidermis by the point of a lancet, soon produces inflammation and a local eruption. In six or seven days the symptoms of a general disease make their appearance, a fever comes on; and three days after, this fever terminates by a more or less plentiful eruption of small-pox on all parts of the body. These pustules resemble exactly that from which the matter for inoculation was taken, and contain a pus capable of communicating the same disease by the same means to other patients.

The inoculation of the cow-pox is not followed by the same phenomena. Commonly at the distance of three days from the inoculation, and never sooner, when the cow-pock matter is good, but sometimes later, a single pock makes its appearance. In five days time this pock comes to perfection. About the eighth day it is surrounded with a red areolà which is a little painful. It is at last converted into a blackish brown crust similar to that from which the cow-pock matter was taken. Sometimes a slight fever makes its appearance with some swelling of the axillary glands, when the puncture has been made in the arm. The liquid contained in this pustule, if it be taken at the commencement of its formation, is capable, when inoculated, of producing the same phenomena in another person, and this may be continued *ad infinitum.*

From what has been said it is evident that the inoculation of the small-pox produces a real small-pox, while that of the cow-pox has not the same result. Hence the matters introduced are not similar. Of course the theory of one of these diseases and of its inoculation cannot be applied to the other.

The only thing which exists in common between them is that those who have been inoculated by either are henceforth free from all danger of catching the infection of the small-pox. This property, common to the man who has had the small-pox, either naturally or by inoculation, or who has been vaccinated, indicates that a general change has been produced in the whole state of the body, which in all of these cases produces a similar result. This result establishes a difference between the person who has been subjected to these processes and him who has not. The latter is exposed to the infection of small-pox, the former is freed from it.

What is the nature of this difference no body knows; experience alone proves its reality. Experience only can in like manner decide whether a general eruption be necessary, and whether there be any danger when this eruption does not take place. For it is not by theory that such a question can be decided, but solely by a comparison of facts. If the cow-pock matter introduced under the epidermis not only produces the phenomena which have been mentioned above, but likewise leaves a poison in the system which may occasion different severe diseases, observation ought to prove that it does so. Thus the question when properly stated turns out to be merely a question about a matter of fact.

But even when we trust solely to experience and observation, the multitude of circumstances often inappreciable, which may in medicine concur to the same result, and the difficulty of assigning the effects produced to their true causes, occasion of necessity a great deal of uncertainty respecting the consequences deduced from observations. A small number of facts similar to those alleged can only produce probability. It is only by their great number and their constancy that presumption is changed into certainty. In order to appreciate the facts alleged against vaccination, we must compare them with the nature and the sum of the established facts which have rendered the general opinion favourable to it.

Some of the facts alleged against the cow-pox have been borrowed from the work of Dr. Woodville, entitled, *Report on the Cow-pox,* published at London, in 1799, and translated the same year into French by M. Aubert. The late M. Chappon collected in 1803, in a work entitled, *Traité Historique des Dangers de la Vaccine,* every fact which had been stated as unfavourable to the new operation. We find there some remarkable facts which we shall examine; but the greater number of them consist in assertions without details and without proofs, which seem to have been collected with less judgment than prejudice. The author himself, convinced at last of the insufficiency of his proofs, published a retraction of his opinion, which he addressed to the authors of the *Journal de Medecine,* published by M. M. Corvisart, Le Roux, and Boyer, and which at his request was inserted in the number of that Journal for September, 1807, tom. 6, p. 238. Other facts have been published in different books, most of which have been collected and examined by the authors of the *Bibliotheque Britannique.* We shall notice such of them as deserve to be known. Several observations have been communicated to ourselves in particular. Almost all those which we had an opportunity of verifying were occasioned by false or inaccurate reports. The rest offered only facts not very remarkable, and the consequences of which were equivocal. No observation can be of any weight except when it is accompanied by the necessary researches respecting the origin of the virus, respecting the conditions characteristic of the cow-pox, respecting its form, its developement and its effects, respecting the phenomena which have followed it, and respecting the state of the person vaccinated. For our parts we have not intentionally neglected a single fact of any value which has come to our knowledge.

We shall compare with these facts, 1. The results of the correspondence of the Society established at Paris, under the auspices of government in 1804, under the title of the *Society for the Extermination of the Small-pox.* This society having collected the papers of the central committee of vaccination formed in 1799, when vaccination was introduced into France, and having joined to it a very active correspondence, continued to the present time, the knowledge which it has acquired of the effects of vaccination observed in every part of France, forms a very complete collection indeed of facts relating to every part of the subject.* 2. The facts collected in the valuable work, entitled *Bibliotheque Britannique*, which from the year 1798 to the present time has presented to Philosophers, the principal observations made on this subject in every part of Europe, and in all other civilized countries. We have likewise had at our disposal a work published by Dr. Sacco, entitled, *Trattato della Vaccinatione, Milano*, 1809, in which there is the history of the labours of that estimable physician, to spread the practice of vaccination over Italy. To our own observations we shall give no farther importance than that united to the facts contained in these different collections, they served to confirm, in our eyes, consequences already established by other observers. For it is not from the facts observed by one man, however well informed, that complete evidence in such a case can result; but from an agreement in the observations of well informed men made at different times, in different countries, and in different circumstances.

SECOND QUESTION.

Do the facts observed demonstrate that the cow-pox introduced into the system, is of such a nature as to produce eruptions, or accidents which ought to be ascribed to the difficulty, the imperfection, or the want of eruptions?

It has been thought that the eruptions which sometimes follow vaccination demonstrate the truth of this opinion. To the want of sufficient energy to produce such eruptions, have been ascribed the dreadful diseases which have been observed to follow vaccination.

We find observations of this sort in the work of Dr. Woodville, published in London, in May 1799, giving an account of the cases of vaccination performed by him in 1798, just after the discovery of Dr. Jenner. In his observations we observe eruptions preceded or accompanied by fever, anxiety, pain of the bowels, vomiting, diarrhœa, fainting, pain and redness of the eyes, cough and convulsions. The same symptoms were observed by him without eruptions, and in such cases they were ascribed to the want of power in the system, to produce the necessary eruption. He describes likewise, an eruption accompanied by spasm, and followed by the death of the patient, an infant at the breast.

* The results of this correspondence are to be found in a report of the central committee, published in 1803; in two reports made at a general meeting of the society in 1804 and 1806; in two others, one for 1807 and 1808, the other for 1808 and 1809; in notes communicated from the report for 1810, at present printing; and in bulletins of correspondence, published hitherto, amounting to 20.

To estimate the value of these observations, and of the consequences which may be drawn from them, we must attend to the history of the observations of Dr. Woodville, and of the different circumstances when inoculation with cow-pock matter was followed by different kinds of eruptions.

Dr. Woodville was chief physician to the London Small-pox Hospital. He inoculated likewise both in the city and in the country. His work was published in 1799, and relates almost entirely to observations made in 1798, just after the epoch of the original discovery of Jenner. The total number of cases given by Woodville amounts to 510. In 274 of these there was an eruption more or less abundant, and in 147 of them there was a fever more or less remarkable.*

At the same time, however, Dr. Jenner announced that the inoculation with the cow-pock matter produced no eruption. He had never observed any: and the physicians who employed the new matter, both in London and in other parts of England, affirm the same thing.†

Dr. Woodville having sent to Dr. Jenner cow-pock matter collected in an hospital, and having received a quantity of other matter from Jenner; the matter sent by Woodville inoculated into more than 60 persons, in Berkeley, and the neighbourhood by Dr. Jenner, and other physicians, produced no eruption; while on the other hand, the matter which Woodville received from Dr. Jenner, though it had occasioned no eruptions when employed by Dr. Jenner, produced them anew when used by Dr. Woodville.‡

Thus the phenomenon was confined to Dr. Woodville. It neither depended upon the virus nor upon any thing peculiar in London.

A new observation was soon after made by Dr. Woodville himself. The eruptions gradually began to disappear in his hospital, when patients inoculated with the small-pox no longer remained in it. The rate of disappearing evidently points out the origin of the eruption. The eruptions observed in 310 persons gradually reduced themselves to 19 per cent., to 13 per cent., to 7 per cent., and at last to 3 or 4 per cent. While in persons vaccinated in the city, he observed no eruptions whatever.§

* Bibl. Brit. vol. ix. p. 394; xii. 163, 298, 325.

† See the work of Dr. Woodville translated by M. Aubert, and the Bibl. Britannique, *scientific department*, vol. xii. p. 146, 163, 172, 173, 272. Pearson's *Observations concerning Eruptions*, extracted in the Bibl. Britan. vol. xiv. p. 254. Jenner's *Enquiry into the Causes and Effects of the Variolæ Vaccinæ*, London, 1798, extr. Bibl. Britan. vol. ix. p. 367, 394. Correspondence of Dr. De Carro, and report of Dr. Woodville, ibid. vol. xii. p. 163, 290.

‡ Bibl. Brit. vol. xii. p. 293, 325; xv. 367.

§ Observations on the Cow-pox, Woodville, London, 1800, extr. Bibl. Brit. vol. xv. p. 370.

It was observed at the same time, in some villages in the neighbourhood of London, where the small-pox was epidemic, that eruptions appeared anew, as attendants of vaccination. The same thing was observed at Ketley, in Shropshire, in a house where there were a considerable number of persons inoculated with the small-pox.*

Finally, Dr. Jenner in a letter to Dr. Marcet, dated 25th February, 1803, affirms that out of 10,000 persons innoculated in England by himself and his nephews, not a single person had been afflicted with eruptions.†

In 1807, the report of the Royal College of Surgeons in London announced, that out of 164,381 vaccinations, 66 persons only had shewn any eruptions. This is in the proportion of 1 to 2490·6.‡

These observations made in England are confirmed by similar observations made in other countries.

When the practice was introduced into Denmark eruptions were perceived which afterwards disappeared.§

The same thing was observed at Hanover and Geneva. The observations made at Geneva deserve particular attention. It was in 1800 and 1801, while the small-pox was epidemic, that eruptions were first perceived during the course of vaccination. Afterwards they disappeared; but in 1808 the small-pox contagion was introduced again. The eruptions after vaccination made their appearance a second time. Since that time they have not been observed.‖

One of ourselves observed the same thing at Lucca in the month of July, 1806. The small-pox was epidemic, and among the children vaccinated at that time several had eruptions, which were not perceived afterwards.**

In the correspondence of the Society formed at Paris, there are examples of sporadic eruptions; and the number of cases in which they appeared bears no proportion with the cases of vaccination practised in the empire. That quantity from the last six months of 1804 to the end of 1810 amounts to 2,671,661 vaccinations.††

The nature of the eruptions observed has been very variable. In general the pocks resembled those of the chicken-pox rather than those of the small-pox. Some have resembled the cow-

* Bibl. Brit. vol. xv. p. 371.

† Bibl. Brit. vol. xxv. p. 182.

‡ Bibl. Brit. vol. xxxvi. p. 371.

§ *Dr. Jenner's letter to Dr. Marcet*, Bibl. Brit. vol. xxv. p. 182.

‖ Odier Bibl. Brit. vol. xx. p. 214; xxxix. 91, 93, 94; xlv. 64, 65.

** *Memoir of the Class of Physical and Mathematical Sciences of the Institute*, vol. viii. p. 21.

†† Notes communicated to the Secretary of the Society established at Paris for the extinction of the small-pox.

pox, and some physicians even affirm that they have communicated the true cow-pox by inoculating with the liquid which they contained.* In other cases they have resembled a miliary eruption. They were hard, and contained no vesicles; sometimes they amounted to nothing more than red spots, or blisters.† We might refer likewise to the number of consecutive eruptions observed, in cases of vaccination, the secondary cow-pox either appearing upon the same place as the first, or in other parts of the body, if it were not demonstrated, in a great many cases, that the children have produced them themselves, by scratching different parts of their body after having broke the pock produced by inoculation. Those pocks which have the closest resemblance to small-pox or cow-pox have always been observed to be more fugitive than the true cow-pox or small-pox.‡

It follows from the preceding detail, that the cases where eruptions and fevers have taken place after vaccination, compared with those in which they have not taken place, are in so small a proportion that they cannot be referred to the cow-pox virus, or regarded as a consequence of its properties. They can only be referred to accidental circumstances, either general or individual. Though these circumstances cannot always be pointed out in a particular case, yet the greater number of them, especially when a great many eruptions have appeared at the same time, are obviously connected with the existence of the small-pox in the places where vaccination is practised. Hence we have no proof whatever that vaccination introduces into the body a poisonous ferment, which ought to be expelled by fever and eruptions. The very opposite inference ought to be drawn from the very great number of cases where vaccination has produced no sensible change, except in the very part where the inoculation was performed, and has neither occasioned fever nor general eruption.

(*To be continued.*)

Article X.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Mr. Cavendish.*

In our account of the scientific labours of this celebrated philosopher in our last number, we inadvertently omitted to notice one of his papers. It was the last paper which he ever

* Bibl. Brit. vol. xv. p. 86, 369; xxxix. 94.

† Bibl. Brit. vol. xv. p. 82, 369, 370, 379; xxxix. 93.

‡ Bibl. Brit. vol. xiv. p. 258, 260; xvi. 203, 297, 299, 300.

wrote; and, unless we are mistaken, (for we have not the volume at hand), it was published in the Philosophical Transactions of 1809 or 1810. It gives an account of his method of dividing astronomical instruments, and was published in consequence of Mr. Troughton's paper on the same subject. Mr. Cavendish's paper makes us acquainted with an improvement which he made in the method of using the beam compasses.

II. *Gold.*

No metal has been longer known, nor in higher estimation, than gold: yet there are few whose chemical history is so incomplete. Nothing is easier than to obtain gold in a state of purity, and to examine its properties when in that state. Accordingly its colour, specific gravity, fusibility, and inalterability (if the word may be permitted), in the air, have been long ago determined with precision. We are also, in consequence chiefly of Mr. Hatchett's experiments, pretty well acquainted with the alloys which it forms with other metals; but its combinations with oxygen, with chlorine, and with the combustible bodies, and the salts which its oxides are capable of forming with the acids, are almost quite unknown. On that account I think it will be interesting and useful to collect here all the facts respecting these subjects that have been ascertained within the last two or three years.

1. It has been long the general opinion that there are two oxides of gold; but neither the experiments of Proust, nor my own, were able to determine the proportion of oxygen in a satisfactory manner. According to Berzelius, these oxides are composed as follows:—

Protoxide	100 metal	+	4·005 oxygen
Peroxide	100	+	11·982

(Lärbok i kemien, ii. 436). According to this statement, which he informs us is founded on his own experiments, if we make the number representing the weight of an atom of oxygen 7·5, then an atom of gold will weigh 168. The protoxide will be a compound of

	Gold.		Oxygen.
	1	+	1 atom
The peroxide of	1	+	3

There ought, therefore, to be an intermediate oxide between those composed of 1 atom of gold and 2 atoms of oxygen. This corresponds so far with my experiments, I came to a similar conclusion.

2. The protoxide of gold is a green powder. The peroxide is yellow. It has a styptic and metallic taste, and occasions a flow of saliva. It is slightly soluble in water; and paper strewed

with it burns as if it had been dipped in a solution of saltpetre. It is scarcely soluble in nitric acid; but muriatic acid dissolves it with facility. This oxide may be procured by the following process:—

Dissolve gold in a mixture of two parts of muriatic acid and one part of nitric acid. The solution ought to be as nearly neutral as possible. When alkalies, alkaline earths, or alkaline carbonates, are dropped into this solution, no precipitate appears; but if the liquid be heated, a portion of oxide of gold falls down. The whole oxide is not precipitated, because the muriate of gold has the property of combining with the alkaline muriate, and making with it a triple salt. (See Vauquelin's experiments, Ann. de Chim. lxxvii. 321.)

3. Gold cannot be combined directly with sulphur; yet there can be little doubt that there is an affinity between the two substances; for sulphuric acid combines with the oxide of gold. It cannot be the addition of oxygen surely, the particles of which repel each other, that occasions the affinity. It must exist between the metal and sulphur. If sulphureted hydrogen gas, or an alkaline hydrosulphuret, be mixed with a solution of gold, a black powder precipitates. This is a sulphuret of gold. It is composed, according to the experiments of Oberkampft, of

Gold	100·00
Sulphur	24·39

Either the analysis of the oxides of gold by Berzelius, or this analysis, must be erroneous; for they do not correspond with each other. It is still farther from agreeing with the following law, which Berzelius has deduced from his experiment:—

"Let 100 parts of any metal be combined with a portion of oxygen sufficient to convert it into a protoxide, and a portion of sulphur sufficient to convert it into a sulphuret. The weight of the oxygen is equal to half the weight of the sulphur."

But there are sufficient reasons, I think, to set aside this law as inaccurate. At present chemists are generalizing too fast. Generalization is the fashion, and it is often adopted upon very slender grounds. I intend to give a striking proof of this in a succeeding number of this Journal.

III. *Putrefaction.*

Some years ago Gay Lussac announced in the Annales de Chimie, and corroborated his opinion by seemingly decisive experiments, that putrefaction does not take place if atmospherical air be completely excluded. Upon this is founded a well-known method of preserving meat fresh for any length of time. Cut it in fragments, fill a bottle with these fragments, plunge the bottle up to the neck in boiling water, and, when boiling hot, cork it so tight as to exclude the air. I have eaten green peas and gooseberries preserved in this way for a year without any perceptible change, either in their appearance or taste. In the last number of Nicholson's Journal (Vol. xxxiv. p. 49), there is a paper by Dr. John Manners, of Philadelphia, in which he relates a series of experiments, demonstrating, as he says, the inaccuracy of Gay Lussac's statement. How far Gay Lussac's experiments are correct, I cannot say, as I have never repeated them; but those of Dr. Manners are quite insufficient, at least to prove them inaccurate. He omits a precaution which Gay Lussac says is essential for obtaining the same results as he did. Gay Lussac says, that the smallest quantity of oxygen gas present is sufficient to produce a commencement of putrefaction; and after the process has commenced, it goes on, whether air be present or not. Therefore it is always necessary to expose the subject of experiment to the heat of boiling water in the first place, in order to separate the small portion of air which adheres to its surface. Now, as Dr. Manners neglected this circumstance, his experiments cannot be considered as conclusive.

IV. *Constituents of Bodies.*

I have been requested by a correspondent to explain why the theoretical numbers given in p. 61 of our last number do not agree with those of Sir Humphrey Davy in his Elements of Chemical Philosophy. The reason is, that Mr. John Davy, from whom the numbers in p. 61 of our Journal were taken, conceives water to be composed of one atom of hydrogen, and one atom of oxygen; and that an atom of hydrogen, of course, weighs 1, and an atom of oxygen 7·5. Whereas Sir Humphrey Davy conceives water to be composed of two atoms of hydrogen and one atom of oxygen. Therefore, as he makes the weight of an atom of hydrogen 1, he makes, of course, that of oxygen 15. If the numbers in p. 61 of our last number be multiplied by 2, they will coincide with those of Sir Humphrey Davy, at least very nearly.

In answer to the request made by the same correspondent, that I should publish a table of the constituents of bodies, and my reasons for adopting the numbers which I shall fix upon, I have to say, that it was my intention to give a table of this kind before I received his letter; but a reason, which I cannot assign at present, but which will appear when our fifth number is published, makes it necessary for me to delay it, at least till our sixth number. Such a table, in order to be useful, must be preceded by an exposition of the atomic, or Daltonian theory. As this theory has hitherto attracted but little of the attention of our London philosophers, except Dr. Wollaston, it may be necessary to enter more fully, both into historical and philosophical details, than would otherwise be proper in a periodical journal. On that account, it is possible that our table of the constituents of bodies may be delayed till our seventh number.

V. *Organs of Digestion of Birds.*

In our account of Sir Everard Home's paper on this subject, in p. 74 of our last number, we have reversed the matter of fact as far as the length of intestines goes. Those birds have the largest intestines whose food is most scanty, and those the shortest intestines whose food is most abundant.

*** *Being desirous of inserting, in this number, the Report of the French Institute, which reached us very late, we are compelled to defer, till our next, several Scientific Notices, for some of which we are indebted to a valued Correspondent.*

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The Society met again on the 14th January, after the Christmas holydays.

The remainder of Dr. Lambe's paper on arsenic was read. He distilled a mixture of quick-lime and oxide of arsenic, and obtained carbonic acid gas, carbonic oxide gas, and some of an inflammable gas which he called nitro-carbonic oxide. There remained in the retort a compound of lime and arsenic; the lime was mostly in the state of carbonate. On one occasion, probably when iron filings were mixed with the lime and oxide, some metallic arsenic was sublimed. He had employed 1340 grains of white oxide of arsenic, and after the experiment could detect only 1040 grains. Hence he inferred, as a conclusion, that a portion of the arsenic had been decomposed, and resolved into carbon, azote, and oxygen.

It is hardly fair to make remarks upon a paper which one has only heard read, as many circumstances must escape observation which materially affect the accuracy of the reasoning; but in this particular instance I hope I may be indulged with an observation or two, on account of the extraordinary consequences which Dr. Lambe has drawn, and which do not appear to be warranted by his experiments, even as he has stated them himself.

1. Dr. Lambe did not examine his oxide of arsenic. For any thing we know to the contrary, it may have contained carbonic acid, or it may have been mixed with chalk.

2. Dr. Lambe did not ascertain whether a quantity of arsenic

reduced to the metallic state was present in the mixture found in the retort. The loss, for any thing that we know, may have been owing to the escape of oxygen gas.

3. Dr. Lambe has not shown that the weight of the gases which he obtained was exactly equal to the loss which he has noted in the oxide of arsenic. We will venture to say that they do not amount to one half of that loss. Therefore it is as fair to conclude that a portion of the arsenic was *annihilated*, as that it was converted into carbonic oxide or acid.

A letter from Col. Humphries, of Connecticut, in New England, to Sir Joseph Banks, was also read, giving an account of a new variety of sheep, which appeared in that state in 1791, and has been propagated. A ram of the variety was originally produced on a farm. The ewes impregnated by this animal sometimes produced the new variety, sometimes not. By degrees, a considerable number of them were produced, and the breed was regularly propagated. It was called the otter, or ankon sheep (from the Greek word for elbow). The characteristic was, very short legs, particularly the fore legs, which were bent somewhat like an elbow. The skeleton, brought to this country, was compared by Sir Everard Home with the smallest Welch sheep that could be procured. The bone of the fore-leg of an ankon sheep weighing 45lbs. was thicker, but not so long, as that of a Welch sheep scarce $\frac{1}{4}$th of the weight. The joints of the ankon sheep were looser knit than usual, and the animal was feebler. Its mutton was as good as usual; its fleece rather worse. In New England the farms are surrounded only with low wooden or stone fences. The ankon sheep was propagated because it could not so easily get over these fences and injure the corn.

The facts contained in this paper are of great interest, in a physiological point of view, by affording a clue to the numerous varieties of various animals that exist over the earth.

On Thursday the 21st a short paper by Sir Everard Home was read, on the coagulating power of the stomach. He endeavoured to determine, by experiment, in what part of the stomach this power resides, and ascribes it to the solvent glands in particular.

LINNÆAN SOCIETY.

The Society met on the 19th January, after the Christmas holydays. The paper read consisted of descriptions (accompanied by figures) communicated (from the executors of the late Mr. Anderson, of St. Vincent) by A. B. Lambert, Esq. V.P.L.S. The first, a new species of psidium (P. polycarpon) ripened fruit last summer in Mr. Lambert's stove. It is a low shrub, with decumbent branches, bearing a round yellow fruit, the size of a large cherry, and of a flavour esteemed superior to that of the common guava. It is a native of the Savannahs of the Island of Trinidad, whence it was introduced into the botanic garden at St. Vincent's in 1792.—The second, a species of brownea, supposed by Mr. Anderson to be new, and in fact first discovered by him in Trinidad in 1786; but since described and figured by Jacquin under the name of brownea capitilla. It is a small tree, with large clusters of bright scarlet flowers, which were unknown to Jacquin.

WERNERIAN NATURAL HISTORY SOCIETY.

The following papers have been read at the last meetings of the Wernerian Natural History Society:

I. A mineralogical description of the Ochil hills; by Charles Mackenzie, Esq. The first part of this valuable paper contained a geographical delineation of that beautiful and interesting range of hills. The second part contained a full and accurate account of all the rocks of which it is composed, beginning with the lowest or oldest, and ending with the uppermost or newest. The following are the rocks described by Mr. Mackenzie, arranged according to their relative antiquity, the first-mentioned being the oldest, the last the newest: 1. Red sandstone. 2. Amygdaloid. 3. Grey sandstone. 4. Limestone. 5. Slate clay. 6. Claystone. 7. Tuff. 8. Basaltic clinkstone. 9. Greenstone. 10. Claystone porphyry. 11. Compact felspar. The third part contained a short account of the different veins that traverse the strata of the district; these are calcareous spar, streatite, heavy spar, iron, cobalt, silver, copper, and lead.

II. A paper by Professor Jameson, on the distribution of carbon throughout the mineral kingdom.

III. A memoir by Professor Jameson, on universal and partial or local formations.

IV. A journal kept in the Greenland seas, during the summer of 1812, by Mr. Scoresly, junr. of Whitby; which contains much curious information: by the same gentleman, a short but interesting communication in regard to the Polar Bear.

V. An account of the old silver mines in Linlithgowshire, by Mr. Fleming: and, by the same gentleman, an account of a great bed of sea shells, observed a considerable height above the high water mark of the Frith of Forth; and likewise an account of the Sorex fodiens, found by M. Fleming, in Fifeshire.

VI. Dr. Yule, in continuation of his physiological observations on mono-cotyledonous plants, read a memoir on the characters of the *Triticeæ* and *Hordeaceæ*, as distinct natural orders. It is singular, he observed, that an assemblage of plants, occupying by far the greatest portion of the surface of the habitable globe, existing under every extreme of temperature, from between the tropics to the vicinity of the poles, should have remained so imperfectly known, as to be still considered as one natural order: for, since the accurate description and delineation of upwards of 400 species, by Scheuchzer, enabled Linnæus to characterize the genera then known, and the addition of others by Konig and Thunberg, with the numerous species described by Leers, Swartz, Schreber, and Brown, no subdivision of these plants has been effected on the principles of the natural method, even that of Jussieu being merely artificial. The present therefore is the first attempt to arrange a part of this extensive series on the principle of natural affinity.

IMPERIAL INSTITUTE OF FRANCE.

Analysis of the Labours of the Class of Mathematical and Physical Sciences of the Imperial Institute during the year 1812.

MATHEMATICAL PART. *By M. le Chevalier Delambre, Perpetual Secretary.*

ANALYSIS.

Memoir on the Attraction of Homogeneous Spheroids. By M. le Chevalier Legendre.

This is the third time that M. Legendre has returned to this difficult problem, which for more than 70 years has occupied various mathematicians of great celebrity. Maclaurin, in his Dissertation on the Flux and Reflux of the Tide, which shared the prize of the Academy of Sciences for 1740, resolved it in a satisfactory manner in the case when the point attracted is within the surface of the elipsoid. He attempted, likewise, the other case, when the point attracted is without the surface; but he could only give the solution of a particular case, namely, when the external point is situated in the prolongation of one of the three axes of the elipsoid. The theorem which he obtained brought this particular case to that where the point attracted is upon the surface of the elipsoid, and at the extremity of one of its axes. M. Le Comte Lagrange had, by an elegant analysis, confirmed all the results of Maclaurin: D'Alembert had demonstrated them in the Berlin Memoirs: M. Legendre, in Vol. x. of the Memoirs presented, gave a new demonstration of them; and in considering generally the attraction of spheroids of revolution upon an exterior point, he succeeded in reducing it to the attraction on a point situated in the axis of the solid, whatever was the figure of the meridian. It remained to be shown how this solution could be extended to all elipsoids. This is what M. Laplace did in his admirable Dissertation on the Figure of the Planets, expressing the attraction of any spheroids whatever in series. Excited by the desire of overcoming more directly a great difficulty, M. Legendre resumed in 1788 the consideration of this subject. He demonstrated that the theorem of Maclaurin for points situated in the prolongation of the three axes might be extended to all the points situated in the plane of any one of the three principal sections of the solid: and considering the problem in all its generality, he showed that the difficulties of integration might be overcome; but he acknowledges, at the same time, that the analysis was exceedingly complicated.

M. Biot, by a happy complication of a theorem of M. Legendre with a complete integral upon which M. le Comte Lagrange had founded his theory of fluids, succeeded afterwards in reducing the attraction of an elipsoid upon an exterior point to the case when this point is situated upon the surface of the elipsoid itself.

After so many efforts, in which all the resources furnished by the most skilful analysis seems to have been exhausted, little hopes were entertained of obtaining a more easy solution. This, however, has been accomplished by Mr. Ivory, who, by a transformation as simple as ingenious, has demonstrated that the attraction of a homogeneous elipsoid upon any external point whatever may be reduced to that of a second elipsoid upon a point within it. "Thus," says M. Legendre, "the difficulties of analysis which the problem exhibited disappear at once; and a theory which belonged to the most abstruse part of mathematics may now be explained in all its generality in a manner almost entirely elementary." The object of M. Legendre, in the new memoir which he has communicated to the Class, is to take advantage of the discovery of Mr. Ivory to present the entire theory of the attraction of homogeneous elipsoids in all the simplicity of which it has become susceptible.

He states, first, the general formulas of the problem which extend to all the points within and without the elipsoid. He then explains, in a very clear manner, the method of Mr. Ivory, which consists in making the surface of a second elipsoid pass through the external point. The principal sections of this second elipsoid are situated in the same planes, and described from the same foci, as the corresponding sections of the given elipse. Then upon the surface of the first elipsoid a point is taken, such that each of its co-ordinates is to the corresponding ordinate of the exterior point, in the same ratio as the analogous semiaxes of the two elipsoids. The point thus chosen will be within the second elipsoid, and we may calculate its attraction parallel to each of the three axes of the second elipsoid. In order to adduce the three attractions of the external point to the first elipsoid, it is only necessary to multiply those of the second by the ratios between the products of the two other axes in the two elipsoids. Hence it is obvious that by a simple multiplication, the second case of the general problem, considered hitherto as extremely difficult, is reduced to the first case, already completely and elegantly resolved. It is only necessary, then, to explain that solution in order to complete entirely the theory of the attraction of homogeneous elipsoids. Here M. Legendre leaves Mr. Ivory, who has only resolved this case by means of series, and adopts the method of M. Lagrange, which reduces the problem to the quadratures, and permits the use of the known method of approximation.

If the elipsoid differ very little from a sphere, then the series which expresses the attractions becomes very convergent; the law by which the coefficients are regulated is easily perceived, and M. Legendre gives the expressions of it.

If we do not chuse to have recourse to series, or if the eccentricities of the principal sections are too considerable to allow the series to converge with sufficient rapidity, then M. Legendre has recourse to transcendental eliptics, of which he has given the theory in his Exercises on the Integral Calculus.

The formulas become much more simple if the elipsoid has two equal axes, or when it becomes a spheroid of revolution; but the formulas are not quite the same for the flat spheroid and the elongated spheroid.

Finally, as notwithstanding the unexpected simplicity which results from the process of Mr. Ivory, the solution is still long, and sufficiently intricate, M. Legendre has collected into a synoptical table all the formulas which must be valued in succession, a table which must be very agreeable to young mathematicians, who will find nowhere a problem so complete, so various, and so accommodated for practice, as this difficult problem,

GENERAL PHYSICS.

Memoir on the Distribution of Electricity on the Surface of Conductors. By M. Poisson.

The hypothesis most generally received at present to explain the phenomena of electricity is that which ascribes them to two fluids; the particles of each of which mutually repel, while the particles of one fluid attract those of the other.

It was according to this hypothesis that Coulomb calculated the phenomena which he had observed, and he succeeded in demonstrating that these attractions and repulsions followed the law of the inverse of the square of the distances. Though this hypothesis has not yet acquired all the requisite certainty, yet it offers such a connection between all its parts as inspires us with confidence, till a complete demonstration be obtained, which can only be the result of the constant coincidence of calculation with the phenomena.

M. Poisson, in the memoir of which we are going to give an account, adopts this hypothesis. His object is to determine analytically the manner in which electricity distributes itself on the surface of conductors; to compare his calculations with accurate observations, to find, if possible, a confirmation of the hypothesis which he adopts.

If all parts of a body contain equal quantities of both fluids, no sign of electricity appears. The body is then said to be in its natural state: in that case, if we introduce a given quantity of either fluid, this fluid added will distribute itself on the surface of the body, where it will be retained by the surrounding air. This is what Coulomb demonstrated. There will be formed, therefore, at the surface of the body a very thin coating, the thickness of which at each point will depend upon the form of the body. It ought to assume the figure proper to produce equilibrium.

M. Poisson proves that the problem may be reduced to find what ought to be the thickness of the coat of fluid in each point of the surface, in order that the action may be nothing in the interior of the excited body. This thickness will be greatest at the summit of the longest of the three axes, and smallest at the summit of the shortest of them; and these thicknesses will be to each other as the lengths of the axes. If we suppose the thickness of this coat to become very small, we shall then have the distribution of electricity on the surface of a spheroid little different from a sphere. This case, and that of an elipsoid, are the only ones in which it is possible, in the present state of the science, to assign the various thickness of the coating of fluid.

By making use of the formulas of the attraction of spheroids, we may calculate the attraction of the coating for a point placed within or without the excited body. By this means M. Poisson has found that, at the surface of a spheroid, little different from a sphere, the repulsive force of the fluid is proportional to its thickness in each point. The same holds at the surface of an elipsoid of revolution, whatever be the ratio of its axes to each other. Thus in these two kinds of body, the electrical repulsion is greatest in those places where the electricity is accumulated in the greatest quantity. It is natural to think that this result is general; but it is very difficult to demonstrate that it is so. M. le Comte Laplace, has demonstrated it in a manner purely synthetical. His demonstration will be found in the memoir. It results from it, that in every case the repulsive force is proportional to the thickness. We cannot conclude that the pressure varies at the surface of excited bodies, and that it is proportional to the square of the thickness. In the places where that pressure surpasses the resistance which the air opposes, the air gives way and the fluid flows as through an opening, this is what happens at the extremity of pointed bodies, and at the sharp edges of angular ones.

The principle which constitutes the basis of all this theory, applies equally to the case of any number of conductors acting mutually upon each other. It will furnish in each case as many equations as we consider of conductors; and these equations will serve to determine the variable thickness of the coating which envelopes these different bodies.

M. Poisson satisfies himself for the present, with giving these equations for two spheres of different diameters, formed of a matter which is a perfect conductor, and placed at any distance from each other. When the two spheres touch, the equations may be integrated in a very simple manner by definite integrals. They show us that the the thickness is nothing at the point of contact, that is to say, that if two spheres of any diameters are placed in contact and exeited in common, there will be no electricity at their point of contact. In this particular the calculus agrees exactly with the experiments of Coulomb. (Acad. des Sciences 1787.)

In the neighbourhood of this point, and to a considerable distance from it, the electricity is very weak upon the two spheres. When it begins to become sensible, it is at first most intense on the largest of the two spheres; after this it increases at the greatest rate upon the smallest sphere, so that upon the point diametrically opposite the point of contact it is always greater on the smaller sphere than in the corresponding point of the greater sphere.

When the spheres are separated, each carries off the whole quantity of electricity with which it was covered, and as soon as they are removed beyond all mutual action, this electricity is distributed uniformly on each sphere. But the ratio of the mean thickness is given by this analysis in a function of the ratio of the two radii. Thus the formula of M. Poisson includes the solution of this physical problem: *To find in what ratio electricity is divided, between two globes that touch, and whose radii are given.* This ratio is always less than that of the surfaces; so that after separation the thickness is always greater on the smaller of the two globes. The ratio between these two thicknesses tends towards a constant limit which is equal to the square of the ratio of the circumference to the diameter, divided by six, which is very nearly 5 to 3. Thus when a very small sphere is placed upon an excited sphere of considerable size, the electricity divides itself between the two bodies in the ratio, of about five times the surfaces of the small sphere, to three times the surface of the greatest.

Coulomb had endeavoured to determine this ratio by experiment. He had always found it below the number 2, or 6 to 3, from which he concluded that the number two ought to be its limit. It is easy to see that such a limit could not be experimentally determined with much precision. Therefore, instead of objecting to the theory its want of coincidence with experiment, we ought rather to be surprised at the near approximation of the formula and these experiments made more than twenty years ago, and which M. Poisson details at the end of his memoir. In reality, the difference never amounts to a thirtieth part of the quantity to be determined.

Hitherto M. Poisson has only considered a single excited body, or several bodies touching each other in such a manner, that the electricity may pass freely from one body to another. He now shows how the analysis applies equally to the case when the two fluids occur at the same time upon the surface of the same body. He makes choice of two spheres, separated by a very great interval, in respect to one of the two radii. If we suppose that the small sphere is not electrified in the first place; but merely by the influence of the great sphere, we find that the electricity, contrary to that of the great sphere, accumulates towards the point that is least distant from it, while the similar electricity accumulates on the opposite point. The contrary electricities in these two points are almost equal, and the line of separation differs little from the grand circle perpendicular to the line that joins the two centres and divides the little sphere into two equal parts. Calculation gives by means of very simple formulas, the quantity and the kind of electricity in each point of the two surfaces. The memoirs of M. Coulomb furnish no experiments to which these formulas may be directly applied. But we find in them a curious fact which connects itself with these formulas, and furnishes a new confirmation of the theory.

We shall not enter into any detail respecting this fact, for which we will refer to the Memoir of M. Poisson. We would not even have undertaken the extract which has been just read, were it not that this analysis of the labours of the Class is destined to appear separately. For M. Poisson has himself prefixed to his paper a clear and precise introduction, in which will be found every thing necessary for a philosopher, who is not familiar with the transcendental calculus. This clearness which indicates a mind superior to the subject, is equally conspicuous in the developement of the analysis. He every where points out how far his theorems agree with experiment. This kind of demonstration would not be useless even though the truth of the fundamental hypothesis were quite certain. It is the only one which can ever give confidence to those who, though capable of calculating a formala, are not able to follow its demonstration. It may encourage a taste for analysis by showing that it is a light capable of dissipating the darkness which still covers very important parts of physics. Our readers will learn with pleasure that M. Poisson means to continue his researches, and to extend them to more complicated cases, the means of verifying which he will also find in the Memoirs of Coulomb.

OPTICS.

In giving an account last year of the researches of M.M. Malus and Arago, on various phenomena of optics, we announced that in reporting experiments so new and so delicate, we would make a point of detailing them with the greatest fidelity, and in the very words which the authors themselves employed. After this advertisement which we repeat for what is to follow, we shall obviate an objection that may be made against us. It will be said perhaps that we have somewhat exceeded the bounds of a simple notice. But we beseech our readers to consider that we have to give an account of objects quite new, and of apparatus which could not otherwise be understood so as to be able to repeat the experiments; though this must be an object with more than one philosopher. We shall avoid algebraic formulas as much as possible. Those that we give are short. If we had expressed them in words, our language would have appeared very extraordinary, and it would have been less intelligible, and much more inconvenient for all those who will take the most interest in this new theory.

(To be continued.)

Article XII.

New Patents.

Henry Osburn, of Bordesley, near Birmingham, in the county of Warwick; for a new method of welding and making various kinds of cylinders of iron and steel. Dated November 28, 1812.

Charles Price, of the Strand, in the county of Middlesex, umbrella maker; for a parasol and an umbrella on an improved construction, which he denominates "The Improved Solumbra." Dated December 4, 1812.

Samuel Smith, of Coventry, in the county of Warwick, watch-maker; for an improved escapement for watches, by an invention calculated to make them beat dead seconds from principle, and parts of seconds, by means of clock pallets attached to a lever, operating on a vertical wheel. Dated December 9, 1812.

Robert Were Fox, the younger, of Falmouth, in the county of Cornwall, merchant; and Joel Lean, the younger, of the parish of Budock, near Falmouth aforesaid, Gentleman; for their certain improvements on steam-engines, and the appaitus needful or expedient to be used with the same. Dated December 10, 1812.

JOHN SPENCER, of Port Ballantras, in Ireland, salt manufacturer; for an addition to, or improvement in, the setting up of salt-pans. Dated December 14, 1812.

JOSEPH HAMILTON, of the city of Dublin, Gentleman; for certain new methods of applying well-known principles in the construction and formation of earthen wares. Dated December 8, 1812.

THOMAS ROGERS, of the city of Dublin, Esq.; for a method of applying manual powers to the crane, pile-driver, and other machinery. Dated December 19, 1812.

JOHN HANBURY, the elder, of Bartlett's-buildings, Holborn, in the city of London, warehouseman; for a method of weaving carpets, commonly called Scotch or Kidderminster, by which a new and firmer texture, and larger patterns, can be produced. Dated December 19, 1812.

GEORGE HEFFER, of Carlisle-place, Lambeth, in the county of Surrey, coach-maker; for an improvement in the construction of four-wheeled carriages. Dated December 19, 1812.

JOHN FISHER, of Oundle, in the county of Northampton, iron-monger; for an article for preventing chimneys smoking, which he has named a Smoke-Conductor, and which may be manufactured either in cast iron, wrought iron, copper, brass, and tin, or any other metallic substance. Dated December 19, 1812.

JOHN MORGAN, of York-street, in the city of Dublin, Doctor of Medicine; for a new power applicable to the propelling of vessels and boats of every description through the water, and also to the pumping of them. Dated December 19, 1812.

JOHN LEWIS, of Llanelly, in the county of Carmarthen, assayer of metals; for certain improvements in the art of smelting copper ore. Dated December 19, 1812.

JACOB SAMUEL ESCHANZIER, of Gibraltar, Esq.; and HENRY CONSTANTINE JENNINGS, of Marchmont-street, Russel-square, in the county of Middlesex, Gentleman; for a new mode of manufacturing, using, and applying, certain articles, by means of which mariners and other persons may be saved from drowning. Dated December 19, 1812.

JOHN BARBER, of St. Mary-street, Portsmouth, in the county of Southampton, sword-cutler and surgical instrument maker; for a new instrument of great practical utility to surgeons, namely, an instrument whereby they may with the utmost facility, stay and prevent the hemorrhage of the subclavian artery safely in cases when necessary to amputate the arm from the shoulder joint. Dated December 21, 1812.

Article XIII.

Scientific Books in hand, or in the Press.

Dr. Thomson will shortly publish an Account of the present State of Sweden, from a Journey performed by himself during the Autumn of 1812, principally with a View to Geological and other Scientific Researches.

Mr. Neil, Secretary to the Wernerian Society, will publish his Translation of Daubuisson's celebrated Work on Basalt, early in February. It will be accompanied with Notes by the Translator.

The Rev. Mr. Fleming, Member of the Wernerian Society, has finished his Translation of Blumenbach's interesting Treatise, entitled "Specimen Archaiologiæ Telluris." It will be accompanied with Illustrations drawn from the Petrifactions of the Scottish Strata; and will be published in February.

The First Part of the Second Volume of the Memoirs of the Wernerian Society is in the Press.

A Translation, with Notes, by an eminent Mineralogist of the Wernerian Society, of Cuvier and Brongniart's Mineralogical description of the Country around Paris, is preparing for publication.

Professor Jameson is printing a New and much enlarged Edition of his System of Mineralogy.

Shortly will be published, in One Vol. Royal 4to., Dillenii Historia Muscorum, editio altera cui subnectitur Appendix, in qua indicantur nomina hodierna plantarum a Dillenio descriptarum.

A Treatise on the Motion of Rockets, with the Theory and Practice of Naval Gunnery, by W. Moore, of the Royal Military Academy, Woolwich, will be published in the course of the present month.

Sir James Hall is about to publish an Essay on Gothic Architecture.

Mr. Bullock, proprietor of the Liverpool Museum, has announced a splendid Work relative to the recent discoveries in Natural History, with Plates from Subjects principally in his own Collection.

Mr. Stevenson, Oculist to the Princess of Wales, is about to publish a Treatise on the Cataract.

Mr. Charles Fothergill has in the Press an Essay on the Philosophy, Study, and Use of Natural History.

Dr. Thomas Young has prepared an Introduction to Medical Literature, including a System of practical Nosology.

Mr. H. Smithers, Colliery Surveyor, of Bristol, is about to publish Reports on the Strata of Great Britain, with particular relation to the Limestone, Iron, and Coal Strata.

Mr. J. H. Wishart is translating Scarpa's Treatise on Hernia, from the Italian.

The Library of Mrs. Anne Newton has been announced for Sale, by Messrs. Leigh and Sotheby: it comprises chiefly the Collection of the great Sir Isaac Newton.

*** *Early Communications for this Department of our Journal will be thankfully received.*

Article XIV.

METEOROLOGICAL JOURNAL.

312.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.		Rain.	
1 Mo.										
ov. 25	S W	29·89	29·80	29·845	48	35	41·5			☽
26	E	30·07	29·77	29·920	47	43	45·0		0·16	
27	N	30·21	30·10	30·155	49	42	45·5			
28	N E	30·10	29·92	30·010	47	38	42·5			
29	S E	29·89	29·85	29·870	49	41	45·0			
30	S	29·95	29·88	29·915	50	47	48·5		0·15	
th Mo.										
Dec. 1	S	29·96	29·72	29·840	52	44	48·0		5	
2	N W	30·08	29·96	30·020	49	42	45·5		—	
3	E	30·11	30·09	30·100	49	45	47·0		—	●
4	E	30·08	30·05	30·065	48	38	43·0		0·11	
5	E	30·29	30.08	30·185	44	33	38·5			
6	N E	30·51	30·29	30·400	42	26	34·0			
7	N E	30·51	30·41	30·460	35	23	29·0			
8	N E	30·41	29·96	30·175	34	18	26·0			
9	W	29·96	29·94	29·950	35	24	29·5			
10	N W	29·89	29·78	29·835	34	29	31·5		—	
11	E	30·00	29·97	29·985	36	27	31·5			☾
12	N E	29·97	29·79	29·880	32	24	28·0			
13	N E	29·79	29·71	29·750	34	24	29·0			
14	N E	29·71	29·66	29·685	35	28	31·5			
15	E	29·66	29·20	29·430	34	28	31·0			
16	E	29·20	28·98	29·090	34	28	31·0		—	
17	E	29·22	28·98	29·100	35	32	33·5		0·27	
18	E	29·51	29·22	29·365	38	33	35·5		0·18	○
19	E	29·57	29·47	29·520	38	35	36·5			
20	E	29·76	29·57	29·665	36	31	33·5			
21	N W	29·82	29·76	29·790	38	32	35·0		—	
22	Var.	30·02	29·82	29·920	42	33	37·5		—	
23	N	30·30	30·02	30·160	36	31	33·5		—	
24	N	30·46	30·30	30·380	35	32	33·5		3	
		30·51	28·98	29·882	52	18	36·68		0·95	

he observations in each line of the table apply to a period of twenty-four s, beginning at 9 A. M. on the day indicated in the first column. A dash otes, that the result is included in the next following observation.

REMARKS.

Eleventh Month. 28. The sky about sunset was overspread with *Cirrus* and *Cirrostratus* clouds, beautifully tinged with flame colour, red and violet. 30. a. m. The sky again much coloured.

Twelfth Month. 5. The weather, which has been hitherto mostly cloudy, with redness at sunrise and sunset, begins now to be more serene. 6. Hoar frost. 7. A little appearance of hail-balls on the ground. 8, 9. Clear: hoar frost. 11. Snow this morning, and again after sunset. 13. An *orange coloured band* on the horizon this evening. (This phenomenon arises from reflection by the descending dew.) 15. A gale from N. E. unaccompanied by snow, came on early this morning. 16. a. m. The wind has subsided to a breeze, and there now falls (at the Temp. of 27·5°) snow, very regularly cristallised in stars. 17. a. m. It snowed more freely in the night, and there is now a cold thaw, with light misty showers. 18. A little sleet, followed by snow. Ice has been formed, in the night, by virtue of the low temperature which the ground still possesses. A wet evening. 21. A little rain. a. m. 22. A dripping mist. 24. Cloudy: a little rain: some hail-balls in the night.

RESULTS.

Prevailing winds Easterly.

Barometer : greatest observed elevation 30·51 inches;
Least . . . . .28·98 inches;
Mean of the period . . . .29·882 inches.

Thermometer : greatest elevation . . . . .52°
Least . . . . 18°
Mean of the period . . . . 36·68°
Rain and Snow . . . . 0·95 inches.

The evaporation of this period has not been ascertained.

Plaistow, L. HOWARD.
First Month, 7, 1813.

ANNALS

OF

PHILOSOPHY.

MARCH, 1813.

Article I.

*Biographical Account of Mr. Karsten, of Berlin, Councellor of State, and Knight.**

DIETRICH Ludwig Gustav Karsten was born on the 5th of April, 1768, at Butzow, in Mecklenburg. His father was Wenzeslaus Johann Gustav Karsten, Professor and Teacher of Mathematics in Halle: his mother was Sophia Charlotte Wolfrath. Both died long ago, as well as his only sister, who was wife of the famous Professor Gren, in Halle. Of his relations there still survive his wife Amalie, to whom he was married in 1793, and his two daughters, Amalie and Mariane.

The first part of his education was carried on in his father's house. As he came to Halle in 1778 with his parents he was sent to the public school of that place. In the year 1779 he went to the Pedagogium, where he remained till 1782, and was keenly engaged in the study of mathematics. In 1782 he went to the Mining Academy of Freiberg, where he attended the lectures of those eminent professors Charpentier, Werner, Lempe, Gellert, and Köhler, on oryctognosy, geognosy, economical mineralogy, theoretical mechanics, metallurgic chemistry, assaying, mining, and the laws respecting mining. In this celebrated school he continued for four years, and added prodigiously to his knowledge and his skill in all those departments of science to which he directed his attention. On the 21st of November, 1783, he was named by the late Minister Von Heinitz one of the Royal Mining Pupils.

In 1786 he returned to his father's house at Halle, and attended the different professors in that university; namely,

* Translated from the Gesellschaft Naturforschender freunde zu Berlin Magazin, for 1810. Preface.

Wolf, Krause, Eberhard, Gren, Sprengel, Nettelblatt, Fischer, Forster, and Rudiger; and by his talents and industry gained so much reputation, that King Frederick William II., upon the death of his father on the 17th February, 1787, named him a Mining Cadet, and assigned him an allowance to support him in his university and mineralogical studies.

The same year he published the second part of his father's physico-chemical treatises. His own treatise, written in consequence of the prize proposed by the naturalists of Bern for Michaelmas, 1787, for the best classification and description of clay-slates, horn-slates, and wacke, was successful. The philosophical faculty of Halle conferred upon him the title of Doctor, and the Society of Naturalists in the same place elected him a member of their learned body.

In the year 1788 Karsten went to Marburg, to draw up a complete systematic description of the natural history cabinet of the late Leske. He laid the result of his labours before the eyes of the learned world the year following, in two works which laid the foundation of his reputation, and contributed materially to complete his practical knowledge of the kingdoms of nature which he had undertaken to describe. The names of these two works were:

1. *Museum Leskeanum regnum animale* 364, 8vo. mit Leskens vorausgesetzter kurzer Lebensbeschreibung (with Leske's short descriptions prefixed).

2. *Des Herrn Professor Leske hinter lassenes Mineralien kabinet systematisch geordnet und beschrieben von D. L. G. Karsten, 2 theile*, 8vo. (Professor Leske's mineral cabinet, systematically arranged and described by D. L. G. Karsten, in two parts, 8vo.)

In 1789 he was appointed, by the mining administration of the time, an assessor, with a vote. On the 19th of February, 1792, he obtained the patent of Mine Counsellor. On the 3d of February, 1797, he was appointed Supreme Mine Counsellor (Ober-Bergrath). On the 1st of March, 1803, Supreme Privy Mine Counsellor (Geheimen Ober-Bergrath): and lastly, a little before his death, he was named Counsellor of State (Staatsrath); and the direction of the mines under the Minister of the Interior was committed to his charge. On the 18th of January, 1810, he was named by the King a Knight of the Red Noble Order of the Third Class (des rothen Adlerordens 3ter Klasse).

Such is a short history of the learned career of Karsten, and of the situations which he obtained from the State. There can be no doubt that his universal and well-known merit would have opened him a way to the highest situations. His early death was the more unfortunate for his country, as he was on the point of entering upon an office which was excellently suited to his peculiar attainments.

Karsten's learned labours kept pace with those in which he was engaged on account of the State. As overseer of the Royal Cabinet of Minerals, and as teacher of mineralogy, he promoted the advancement of knowledge by similar methods. He bestowed much pains in increasing and improving the collection entrusted to his care. He endeavoured to accomplish this by a most indefatigable and disinterested correspondence through the whole of Germany, and most other countries of Europe; while his endeavours to render himself still better qualified for his numerous lectures were unremitted and most successful.

Karsten likewise promoted the advancement of knowledge by his writings. Besides the works which we have already noticed, besides many small dissertations in the Transactions of more than 16 learned societies of which he was a member, and besides his translations of some foreign books, he laboured with great success towards improving the classification of minerals. His *Tabellar View of Simple Mineral Fossils*, and its continuation in his *Mineralogical Tables improved by the latest Discoveries*, are well known to mineralogists, and constitute a lasting monument of learning and industry. Of the last work he published a second and much improved edition, two years before his death.

Karsten likewise enriched the Transactions of our society* with many learned treatises, which deserve to be particularly noticed as marks of his extensive knowledge and laudable industry.

How much soever we value Karsten as a learned and industrious naturalist, we value him still more as a man of a truly noble character, as a true friend, and an agreeable companion. We were the more shocked at his death, on the 20th March, 1810, because the short illness which preceded that fatal event did not appear likely to lead to any such issue. After his death his merit was publicly acknowledged by the State: the monarch himself wrote a letter of consolation to his disconsolate widow, and humanely took care of her and her family.

Berlin, April, 1811.

Article II.

On the Liquid Gum from Botany Bay. By Thomas Thomson, M.D. F.R.S.

The vegetable substance which constitutes the subject of the following experiments was sent to me by Mr. Knox, of Glasgow, under the name of *Liquid Gum from Botany Bay*.

* The Society of the Friends of Natural History in Berlin, from whose Transactions this life is translated.

Nothing is known, to me at least, respecting its history, except what the ticket attached to it conveyed. The term *gum*, which is applied to it, seems to indicate that it has exuded spontaneously from some tree or shrub; but the properties of it immediately to be described, render this origin extremely problematical. It is an intermediate substance between the two vegetable principles called *extractive* and *tannin* by chemists. Now no instance, so far as I know, of any species of extractive or tannin having exuded spontaneously from plants can be produced. Indeed, they have all such a tendency to enter into combination, and to form solid compounds with other vegetable principles, that the plant from which any of them could exude spontaneously in a state of purity must be of a very peculiar nature.

The resemblance between this *liquid* gum and kino was so striking as to induce me to examine this last substance. The portion of this drug which is soluble in alcohol is absolutely identical with the solid matter of the liquid gum; but kino contains several substances which are not to be found in our gum. The most remarkable of these is a red substance, which gives a red colour to water, and a still deeper tint to alkaline leys, and which possesses the smell and taste of logwood. It is well known that a part of the kino of the shops comes from Botany Bay, as well as our gum; and from the striking similarity between them, I think there is every reason to believe that both of them are obtained from the same vegetable.

The *liquid gum* (for I shall employ that term, though by no means proper, till we have ascertained its properties) has a red colour, approaching to crimson, but considerably darker. It is opake. It has a peculiar smell, and an astringent taste, without the disagreeable intensity of the infusion of nutgalls. Its specific gravity at 60° was only 1·196. Its consistence in the ordinary temperature of the air is nearly the same with that of turpentine. Like that substance, it is very tenacious, and may be drawn out into very long threads. When exposed to cold it becomes harder; and when heated it becomes more and more liquid, till, at the temperature of about 180°, it may be poured from one vessel to another nearly as easily as water. If the phial containing it be plunged into hot water, and kept corked, the whole gum collects at the bottom, without leaving any stain upon the sides of the phial; but in the open air a solid skin almost immediately collects on its surface. On allowing the gum to cool it becomes as thick as at first.

When a portion of the gum is exposed upon a plate of glass to the open air, it gradually becomes solid, contracts in its dimensions, and splits into very regular fragments. The surface of the gum, thus dried, is as smooth as that of sealing-wax; the fracture is vitreous, and the colour not unlike that of shell lack. It is very brittle, crumbling to powder between the fingers. The loss sustained amounted to 0·45 of the original weight. On exposing the plate of glass on a furnace to a heat of about 212°, the gum sustained an additional loss of 0·11 of the original weight; making the whole loss to amount to ·56.

To learn what had occasioned the loss of weight, 100 grains of the liquid gum were put into a small glass matrass, fitted with a capital and receiver, and exposed to a heat not much exceeding 212°. When the distillation was finished, the dry residue in the matrass weighed 43·5 grains. It resembled in every respect the solid matter left when the liquid gum is exposed to the open air. The receiver contained a quantity of water, weighing 53·5 grains. Thus there was a loss incurred amounting to 3 grains. I ascribe this loss to the escape of moisture; for the receiver was not luted.

The water thus obtained was limpid and colourless. It had the peculiar smell of the gum, and a slight taste not easily described. It did not alter the colour of vegetable blues, nor exhibit any thing remarkable, when treated with the different re-agents. The dry residue smelled like bees'-wax, or at least that smell was diffused through the matrass. This dry residue continues always solid and brittle, and is not affected by any degree of heat not sufficient to produce decomposition.

Thus it appears that *our liquid gum* is a compound of 44 parts of solid matter, and 56 parts of water, and that it is indebted to the water for the property of becoming liquid when heated.

To the 43·5 grains of solid residue in the matrass, 56·5 grains of distilled water were added, to make up the original weight. By long-continued digestion in a heat scarcely exceeding 100°, the water gradually incorporated with the dry matter, and formed with it a compound agreeing in every respect with the original *liquid gum*. By distillation this water was driven off: it had the smell of the liquid gum as before, while the dry residue had the odour of bees'-wax.

Here, then, we have a compound to which chemists have not hitherto paid attention, a combination of water and dry vegetable matter, nearly solid while cold; but becoming liquid by heat, like resin or wax.

Though this *liquid gum* contains so great a proportion of water, it exhibits no strong disposition to combine with any more. When put into water it immediately tinges that liquid of a muddy red, but a portion falls to the bottom; nor was I able to procure a complete solution of it in cold water. Hot water acts better; but a portion precipitates again as the solution cools.

The aqueous solution remains muddy, even after standing for weeks; but by repeated filtrations, or by mixing it with hot water, it may be obtained transparent.

When a drop of water is let fall upon the liquid gum, the spot on which it lights becomes lighter coloured, as if the solid matter consisted of two distinct substances; one, dark coloured, more soluble in water; and the other light coloured, and less soluble: but I found it impossible to procure any two such substances in a separate state, either by means of water, or any other re-agent.

The aqueous solution of our liquid gum is of a dark red colour inclining to brown; its taste is the same with that of the gum; and when evaporated to dryness, it leaves behind it a substance precisely the same as the solid residue procured by exposing the liquid gum to the open air.

Alcohol is the proper solvent of our liquid gum; taking it up very readily, both in its natural state, and when deprived of its water by evaporation: but the solution takes place much more readily in the first than in the second state.

Alcohol of the specific gravity of 0·800 easily takes up half its weight of the gum. The solution is blood coloured, and so deep as to be opake; but a drop or two put into a phial of alcohol gives the whole a beautiful crimson colour.

The solution has an astringent taste, and may be diluted with water without any precipitation. Indeed, the addition of alcohol to the muddy aqueous solution makes it transparent, and prevents all subsidence.

3. Sulphuric ether is slightly coloured by the gum, but dissolves only a very small portion of it.

Thus it appears that the so called liquid gum is a compound of 44 parts solid matter and 56 water; that it is soluble both in water and alcohol, and weak spirits. It remains for us to ascertain the properties of the solid matter. For this purpose I exposed the aqueous solution to a variety of chemical agents. The following are the most important facts,

I. *Action of Glue.*

When a solution of glue is mixed with the aqueous solution of our gum, the mixture becomes immediately filled with flesh coloured flashes, which gradually subside, leaving the solution colourless. If the glue has been cautiously added, as long as any precipitate fall the whole gummy matter of the so called gum is separated; for when the liquid is evaporated to dryness, nothing is obtained but a little glue. When the flesh coloured precipitate thus obtained is dried, it becomes brown, and bears a strong resemblance to the precipitate thrown down from catechu, or nutgalls, by glue. Its colour is lighter, but it possesses all the chemical characters of that precipitate.

If a bit of fresh skin be steeped for some weeks in our aqueous solution, it is converted into leather, precisely as when steeped in the infusion of oak bark.

Hence it follows that the solid matter of our gum possesses the essential character of *tannin.*

II. *Action of the Metalline Salts.*

All the metallic solutions which I tried occasioned precipitates when dropped into the aqueous solution of the gum; but none of them rendered the solution colourless, except nitrate of mercury. The following table exhibits the colour of the precipitate, and of the supernatant liquid:—

	Precipitate.	Liquid.
Nitrate of mercury	Reddish brown	Colourless.
Corrosive sublimate	O	Muddy, and light coloured.
Sulphate of copper	Ochre yellow	Brown.
Sulphate of zinc	Light brown	Brown.
Sulphate of iron	Brown black	Dark green.
Nitrate of silver	Dark grey	Grey brown.
Muriate of nickel	Light yellow	Light yellow.
Muriate of arsenic	Ditto	Ditto.
Muriate of manganese	Grey brown	Grey brown.
Cuprate of ammonia	Dark brown	Dark brown.

These precipitates, when dried, assumed all of them a colour more or less brown, none of which answered well as paints, at least when used with water. The whole of the colouring matter could not be thrown down by these salts. Hence it follows that they form with it compounds to a certain degree soluble in water.

III. *Action of the Earths.*

The three soluble alkaline earths, barytes, strontian, and lime, when dropped into the solution, throw down a beautiful dark brown precipitate in flocks; but the solution still continues coloured, though an excess of the earthy waters be added. When the precipitate is dried it retains its colour, is tasteless, but gives a brown colour to water.

Muriate of alumina likewise renders the solution brown and opake, and a precipitate falls after some hours, which does not appear to be soluble in water. Neither muriate of magnesia, nor muriate of barytes, occasions any change; neither is any precipitate thrown down by silicated potash.

IV. *Alkalies.*

The pure alkalies render the liquid very dark coloured and opake, so that it appears black, though in reality it is only of a very deep brown. The compound does not precipitate with glue, at least immediately.

The liquid sulphuret of potash occasions a copious yellow coloured precipitate.

When an acid is added to the alkaline solution, the original colour is gradually restored, and the liquid becomes again transparent.

V. *Acids.*

The strong acids, namely, the sulphuric, nitric, and muriatic, immediately precipitate a yellow or brown powder, and the liquid retains its deep brown colour.

Oxymuriatic acid immediately destroys the colour, renders the solution transparent, and throws down yellow flakes. The colour is not again restored by adding an alkali.

The tan was destroyed; for the solution being evaporated, yielded only muriate of manganese, sulphate of lime, and a little brown extractive insoluble in water.

VI. *Nature of the Solid Matter.*

The properties above detailed are sufficient to show us that the substance which constitutes the essential part of our gum lies intermediate between extractive and tannin, as they have been described by chemists.

It agrees with tan in being thrown down by glue, in converting skin into leather, and in precipitating most of the metallic salts; but there are two species of tan, differing essentially from each other in many respects.

The first, for the sake of distinction, may be called the tannin of nutgalls, or of oak bark. It is insoluble in alcohol, forms a brown precipitate with glue, and a black with iron. Water is its best solvent. In all these particulars our tannin differs from it.

The second species, which has been discovered by Hatchett, may be called artificial tannin. It is soluble in alcohol, and throws down iron and glue brown. Its taste is astringent and bitter. Our tannin agrees more nearly with this species than the preceding, though the differences are also considerable. Nitric acid does not injure artificial tannin, but it converts the tannin of liquid gum to the bitter principle of Welter.

Extractive is distinguished by being soluble in water, and becoming insoluble by exposure to the air, but continuing soluble in alcohol. It is thrown down in yellow flocks by oxymuriatic acid; it dyes cotton brown. In all these respects the solid matter of liquid gum agrees with it.

Chemists have not hitherto perceived that extractive and tannin of nutgalls stand at the two extremes of a series of substances which gradually pass into each other, and that they are so closely connected that they might all be not improperly arranged under one genus, and receive a common name. Indeed, till this is done the confusion, which at present subsists in this department of vegetable chemistry, cannot easily be removed.

When nitric acid is poured upon the solid matter of liquid gum, it softens and dissolves it with the evolution of a little nitrous gas. The solution has a very dark red colour; but on being heated, that colour rapidly disappears, and the solution becomes a very light yellow. When distilled it diffuses the smell of prussic acid; by continuing the distillation to dryness in a very moderate heat a solid matter is obtained, white if the heat has been a minimum, yellow if a little higher, and brown if the fire has been urged too far. This matter is soft and spongy. It has an intensely bitter taste, readily dissolves in water and alcohol, melts when heated, without undergoing any other change. When the solution of this matter in water is evaporated, small crystals are formed, which detonate when heated. This matter is similar to what has been called the bitter principle of Welter. It is best procured by treating indigo with nitric acid. Its properties have been detailed at full length by Hatchett, Fourcroy, and Vauquelin.

When the tanno-extractive of our gum is exposed to heat it swells, blackens, and melts, and a considerable portion of gas is evolved. The first portion of gas is attended with a dense white vapour, and is absorbed by water so rapidly that I have little doubt that it is ammonia, though I had too little of the gum to be able to verify this supposition by repetition of the experiment. After this the gas obtained is carbonic acid, and carbureted hydrogen. Water comes into the receiver, which is strongly impregnated with a portion of tanno-extractive, not much altered. The charcoal that remains burns to ashes as easily as tinder. The ash that remains is very trifling. It consists, as far as I can judge, of sulphate of lime and carbonate of lime; but other earths might easily have escaped my detection, for the quantity of ashes which I examined did not exceed the hundredth part of a grain.

From the preceding detail it is obvious that the substance which we have been examining is a combination of a variety of tannin and water, and that it has no resemblance whatever to gum.

Article III.

The Description of an Organ by which the Eyes of Birds are accommodated to the different Distances of Objects. By Philip Crampton, Esq.

(With a Plate.)

In the philosophical investigation of the phenomena of vision the eye must be considered under two aspects—as an optical instrument, and as an organ of sense. This mixed nature of the organ subjects the analysis of its functions, upon mathematical principles, to considerable uncertainty; for the perceptions of sight must be considered as depending upon certain motions or affections of the nervous power, which, although originally excited by the agency of light, may exist or be renewed without its presence. In deliriums, for example, the perception of objects that have no existence is as perfect as if their images were formed by rays accurately converging upon the retina. In addition to this, it may be observed, that the eye is not a perfectly achromatic instrument, yet objects are not seen with that kind of indistinctness which might be supposed to result from this imperfection in its optical constitution. It appears, then, that in the present state of our knowledge it must be difficult, if not impossible, to ascertain how much, if any, apparently optical effect is to be attributed to the mechanical constitution of the eye, and how much to the agency of the principle of life. This cannot be better illustrated than by referring to the contradictory opinions which have been maintained among the most distinguished natural philosophers, with respect to the faculty which the eye is thought to possess, of adjusting its focus to the different distances of objects. All the hypotheses which have been framed, to explain the means by which this adjustment is effected, have proceeded upon the supposition that it was necessarily connected with some change, either in the external configuration of the eye, or in the relative position of its internal parts. Now, although it is certain that to form a *perfectly* distinct image upon the retina, the focus of the eye must be accommodated to the distance of the object, still we have as yet no proof that such a *perfect* image is essential to distinct vision.

The nervous influence, whatever may be its nature, which conveys to the sensorium a knowledge of those properties of bodies which are the proper objects of vision, may be as perfectly excited by rays possessing one degree of convergency as another; for we cannot entertain the gross conception of the mind sitting behind the eye to contemplate the pictures which are painted upon the retina. The impressions of light and colours upon the eye, like the impressions of articulate sounds upon the ear, suggest the things which they signify, not by any likeness or identity of nature, but only by an habitual connection, which constant experience has proved to subsist between them; nor is the supposition, which attributes to the eye the faculty of seeing objects distinctly by rays which do not accurately converge upon the retina, altogether without foundation, since M. de la Hire* has demonstrated that "the eye does not change its conformation, or adapt itself to the distances of objects, when they are viewed through a perforated card." For if a small object, placed at that distance from the eye at which vision is most distinct, be viewed through three pin-holes, so disposed that the interval between the most distant of them shall not exceed the diameter of the pupil, the object will be seen single; but if the object be brought either within or beyond the limits of distinct vision, it will be seen multiplied as many times as there are holes in the card, and each of the three images will be as perfect as the single one. It is obvious that the three images are formed by three pencils of rays, which are cut by the retina, either before their convergence or after their decussation, and consequently the object is seen distinctly by rays which do not accurately converge upon the retina. Now, although there is every reason to distrust M. de la Hire's general conclusion, that the eye is not adapted to the different distances of objects by any change in its optical conformation, still I believe it will be readily admitted that if the reality of such a change be at all questionable, the hypotheses, which have been contrived to explain the means by which it is effected, cannot be received as proofs of its existence. Sir Everard Home and Mr. Ramsden, in the true spirit of philosophical research, endeavoured to bring the matter to the test of experiment; but the only conclusion, which those philosophers felt themselves authorised to draw from experiments conducted with a degree of accuracy of which the subject seemed scarcely susceptible, was, "that a change in the length of the axis of vision, for the purpose of adjusting the eye, is rendered highly probable;"† but since, if it happened at all, it could not exceed $\frac{1}{800}$th part of an inch, and as such a change would not account for the phenomena, it became necessary to introduce the agency of other causes, which, as they could not be subjected to the test of experiment, must be considered as merely hypothetical. The strongest argument in favour of the internal changes of the eye seems to be drawn from comparative anatomy; for if it be true that there is in the eyes of birds an organ which regulates the focal distance of the

* M. de la Hire, Journal des Sçavans, 1693. Porterfield on the internal motions of the eye.

† Croonian Lecture, Phil. Trans. 1796.

crystaline lens, then (resting upon the uniformity in nature's laws) we may be assured that in *all* animals the eye is accommodated to the different distances of objects by some change in its optical constitution, although the precise nature of that change, or the means by which it is effected in the various classes of animals, may for ever elude our research. I believe Derham* first entertained the opinion which ascribes to the marsupium, or pecten plicatum, in the eyes of birds the function of regulating the focal distance of the eye. The opinion has lately been revived by Sir Everard Home, but it seems liable to insuperable objections; for, 1st, the marsupium does not exhibit the least trace of muscular structure. I have very carefully examined this organ in the ostrich and the eagle: in these great birds the part is of such a size that were it really muscular the fibrous structure could scarcely escape detection.†

2d. In many birds, as in the turkey, jackdaw, &c. the marsupium terminates in the substance of the vitreous humour, and has no direct attachment to the lens.‡

3d. In all birds it is situated obliquely with respect to the lens, so that if it acted at all it could merely communicate to the lens a motion of rotation, or remove it a little from the axis of vision: this cannot be better illustrated than by referring to Sir Everard Home's excellent representation of this organ (Phil. Trans. 1796). It would seem, then, that we are ignorant not only of the means by which the optical constitution of the eye is so changed as to be accommodated to the different distances of objects, but that we have not hitherto had satisfactory evidence that any such change takes place.

Such I believed to be the state of the subject, when in the month of February last an opportunity occurred to me of examining the eye of an eagle, and shortly after that of an ostrich. In those great birds we can view all the peculiarities which distinguish organs of vision in the feathered tribe in general, rendered large and conspicuous. To this circumstance I am indebted for the discovery of an organ, which I trust will enable us to solve a problem in optical science, which has long occupied the attention of some of the most distinguished members of the Royal Society.

The organ to which I allude is a distinct muscle, which arises from the internal surface of the bony hoop of the sclerotica, and is inserted by a tendinous ring into the internal surface of the

* Physico-Theol. note to p. 106.

† See Plate 3d. In the ostrich the marsupium measured $\frac{8}{10}$ of an inch from the apex to the base, and the longest diameter of the base itself measured $\frac{7}{10}$ of an inch.

‡ Blumenbach's Comparative Anatomy.

cornea about one line within its circumference. In order to demonstrate this muscle it is necessary only to remove the anterior segment of the eye just behind the bony hoop, and then the pigmentum nigrum being carefully washed away, the iris is to be gently detached from the ciliary circle, and the choroid coat from the sclerotica. Some delicacy is necessary in performing this part of the operation, for the muscular fibres adhere to the internal surface of the choroid coat, as well as to the bony hoop: if the choroid, therefore, be not slowly and carefully detached, many of the muscular fibres will be separated from the bone, and confounded with the membrane and its pigment.

When the muscle is thus exposed, its descending fibres will be seen terminated in a well-defined tendinous ring, which advances a little beyond the circumference of the cornea, to which it firmly adheres. The thickness of the muscle, as well as the manner of its insertion, may be most conveniently demonstrated by cutting the anterior segment of the eye through its diameter. The fibres will then be seen upon that part of the cut edge which corresponds with the bony hoop. To complete the demonstration, a pin, or thin probe, may be passed between the muscle and the sclerotica. The nerves which are seen branching in a singularly beautiful manner through the substance of the muscle,* are derived from the lenticular ganglion, a mere inspection of the attachments of this muscle will be sufficient to suggest its action; for since the bony hoop, from which the fibres arise, must be considered as a fixed point, the cornea into which they are inserted must be drawn inwards by their contraction; but the matter admits of demonstration. By means of the galvanic influence, the action of the muscle may be excited in the eye of a turkey a few minutes after the head has been separated from the body, when it may be observed that every contraction of the fibres is attended with a corresponding motion of the cornea; or if the fibres be drawn upwards by means of a forceps, the cornea may not only be flattened, but its convexity may be made to *respect* the iris. Since, then, it may be demonstrated that this muscle is in its action a depressor of the cornea, it seems scarcely necessary to add that its influence must tend to diminish the convexity of the eye. It seems probable, therefore, that the eyes of birds are in the ordinary state possessed of a high refractive power; and an eye so constituted seems peculiarly well adapted to the uses of the animal while it rests upon the earth, but when it soars into the middle regions of the air the rays proceeding from objects below must arrive at the eye in lines which may be considered as parallel; consequently to form any thing like a distinct image the focal length of the

* See Plate 3d.

organ must be increased as the divergency of the rays decreases. This adjustment may be perfectly effected by diminishing the convexity of the cornea, and it has been shewn that there is in the eye a muscle to which this function may be assigned.

EXPLANATION OF PLATE III.

This Plate represents the eye of an ostrich, so prepared as to exhibit the muscle of the cornea in its whole extent.

A, A, A, A. The Sclerotica cut into four portions, and thrown back.

B. The Cornea.

C. The Marsupium.

D. The Muscle of the Cornea.

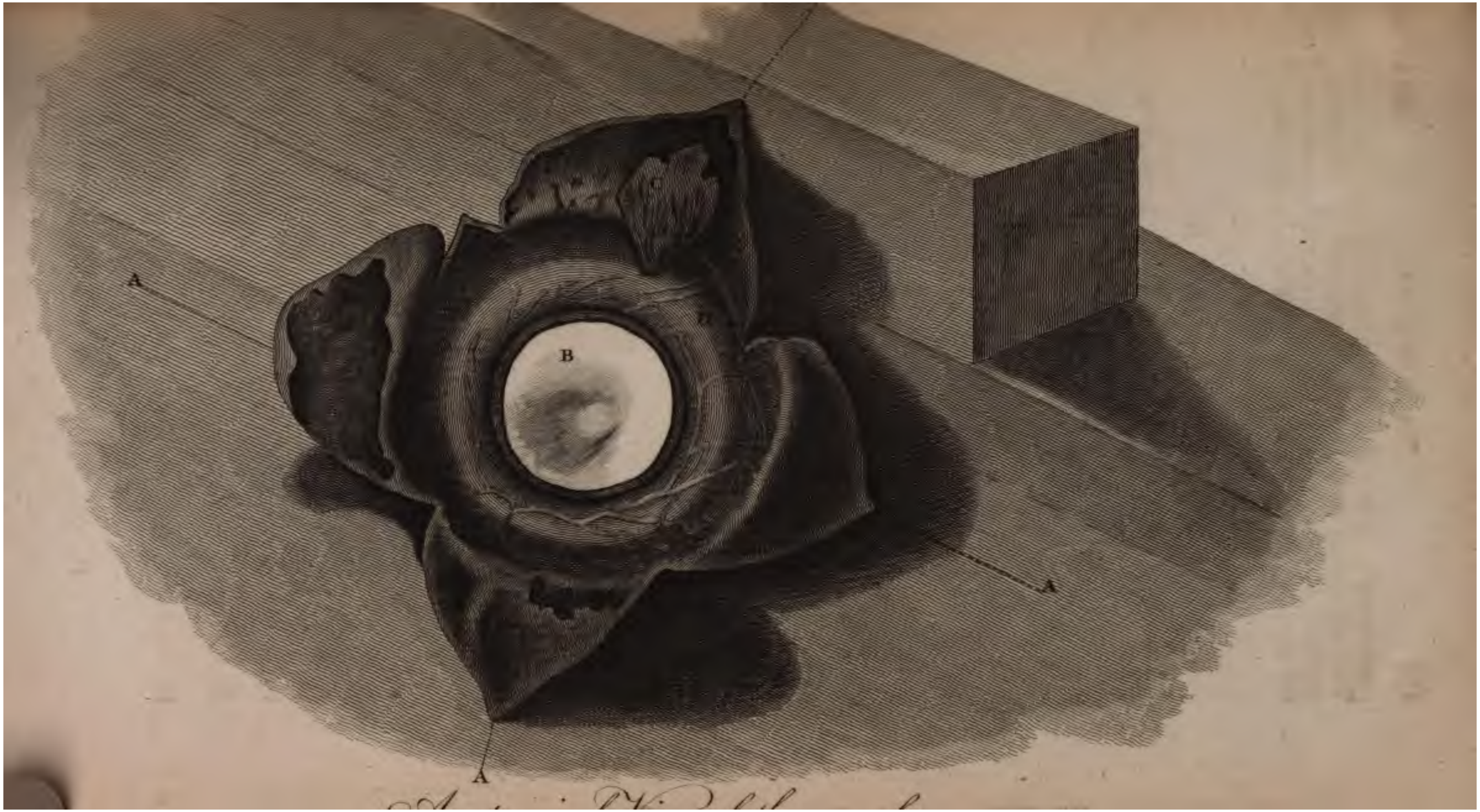
ARTICLE IV.

Population of the principal Towns in Great Britain, according to the Return made to Parliament in 1811.

(See N° I. page 40.)

ENGLAND.

		Inhabitants.			Inhabitants.
1	London	1,050,000	17	Chatham and Rochester	21,722
2	Manchester	98,573	18	Ashton under Line	19,052
3	Liverpool	94,376	19	Exeter	18,896
4	Birmingham	85,753	20	Shrewsbury	18,543
5	Bristol	76,433	21	York	18,217
6	Leeds	62,534	22	Yarmouth	17,977
7	Plymouth	56,060	23	Coventry	17,923
8	Portsmouth, Portsea, Gosport	48,355	24	Great Bolton	17,070
9	Norwich	37,256	25	Preston	17,065
10	Deptford and Greenwich	36,780	26	Woolwich	17,054
11	Sheffield	35,840	27	Wenlock	16,805
12	Nottingham	34,253	28	Shields, North 7,699; Shields, South 9,001	16,700
13	Bath	31,496	29	Oldham	16,690
14	Newcastle upon Tyne	27,587	30	Chester	16,140
15	Kingston upon Hull	26,792	31	Blackburn	15,083
16	Leicester	23,146	32	Wolverhampton	14,836


A
B
A
A

		Inhabitants.			Inhabitants.
33	Wigan	14,060	77	Barton	6,948
34	Dudley	13,925	78	Doncaster	6,935
35	Worcester	13,814	79	Mansfield	6,816
36	Ipswich	13,690	80	Durham	6,763
37	Derby	13,043	81	Beverley	6,731
38	Oxford	12,931	82	Castleton	6,723
39	Saddleworth with Quick	12,579	83	Winchester	6,705
			84	Warwick	6,497
40	Colchester	12,544	85	Chichester	6,428
41	Carlisle	12,531	86	Lewes	6,221
42	Macclesfield	12,299	87	Newcastle under Line	6,175
43	Sunderland	12,289			
44	Whitby	12,218	88	Windsor	6,155
45	Brighthelmstone	12,012	89	Worsley	6,151
46	Warrington	11,738	90	Tynemouth	5,834
47	Cambridge	11,108	91	Ecclesfield	5,805
48	Spotland	10,968	92	Alnwick	5,426
49	Reading	10,788	93	Colne	5,336
50	Lynn-regis	10,259	94	Chorley	5,182
51	Canterbury	10,200	95	Wells	5,156
52	Whitehaven	10,106	96	Haslingdon	5,127
53	Huddersfield	9,671	97	Darlington	5,059
54	Southampton	9,617	98	Litchfield	5,022
55	Maidstone	9,443	99	Gomersall	5,002
56	Lancaster	9,247	100	Bridgewater	4,911
57	Halifax	9,159	101	Newbury	4,898
58	Dover	9,074	102	Stafford	4,868
59	Lincoln	8,861	103	Tewkesbury	4,820
60	Bury	8,762	104	Poole	4,816
61	Northampton	8,427	105	Abington	4,801
62	Gloucester	8,280	106	Ovendon	4,752
63	New Sarum	8,243	107	Weymouth	4,732
64	Boston	8,180	108	Pudsey	4,697
65	Kidderminster	8,038	109	Bedford	4,605
66	Bury St. Edmunds	7,986	110	Stamford	4,582
67	Croydon	7,801	111	Cirencester	4,540
68	Berwick upon Tweed	7,746	112	Battersea	4,409
			113	Bridgenorth	4,386
69	Kendal	7,505	114	Penrith	4,328
70	Pilkington	7,353	115	Ely	4,249
71	Hereford	7,306	116	Knaresborough	4,234
72	Newark upon Trent	7,236	117	Stockton upon Teese	4,229
73	Little Bolton	7,079	118	Ludlow	4,150
74	Scarborough	7,067	119	Falmouth	3,933
75	Peterborough	7,029	120	Tweedmouth	3,917
76	Taunton	6,997	121	Hertford	3,900

4

		Inhabitants.			Inhabitants.
122	Newport (Isle of Wight)	3,855	137	Morpeth	3,244
123	Devizes	3,750	138	Lowestoft	3,189
124	Harwich	3,732	139	Gravesend	3,119
125	Prescot	3,678	140	Richmond (Yorkshire)	3,076
126	St. Albans	3,653	141	Buckingham	2,987
127	Grantham	3,646	142	Guildford	2,974
128	Rippon	3,633	143	Stratford upon Avon	2,842
129	Pontefract	3,605	144	Sandwich	2,735
130	Bridport	3,567	145	Maldon	2,679
131	Monmouth	3,503	146	Shaftsbury	2,635
132	Sudbury	3,471	147	Marlborough	2,579
133	Aylesbury	3,447	148	Huntingdon	2,397
134	Saffron Walden	3,403	149	Northallerton	2,239
135	Sherborne	3,370	150	Thirsk	2,155
136	Andover	3,291	151	Launceston	1,758

WALES.

1	Swansea	8,196	8	Denbigh	2,714
2	Carmarthen	7,275	9	Cardiff	2,457
3	Holywell	6,394	10	Pembroke	2,415
4	Pool	3,440	11	Bangor	2,383
5	Brecon	3,196	12	Cardigan	2,129
6	Dolgelly	3,064	13	New Radnor	1,917
7	Holyhead	3,005	14	Beaumaris	1,810

SCOTLAND.

1	Edinburgh	102,987	19	Ayr	6,291
2	Glasgow	100,749	20	Stirling	5,820
3	Paisley	36,722	21	Irvine	5,750
4	Dundee	29,616	22	Lanark	5,667
5	Aberdeen	21,639	23	Forfar	5,652
6	Greenock	19,042	24	Brechin	5,559
7	Perth	16,948	25	New Monkland	5,529
8	Old Machar	13,731	26	Old Monkland	5,469
9	Dunfermline	11,649	27	Arbroath	5,280
10	Govan	11,581	28	Port Glasgow	5,116
11	Inverness	10,757	29	Alloa	5,096
12	Kilmarnock	10,148	30	Wick	5,080
13	Falkirk	9,929	31	Kirriemuir	4,791
14	Dumfries	9,262	32	Cupar in Fife	4,758
15	Montrose	8,955	33	Dalkeith	4,709
16	Cambletown	7,807	34	Peterhead	4,707
17	Hamilton	6,453	35	Elgin	4,602
18	Musselburgh	6,393	36	Lesmahagow	4,464

	Inhabitants.		Inhabitants.
37 Jedburgh	4,454	59 Bathgate	2,919
38 Kelso	4,408	60 Kirkcudbright	2,763
39 Haddington	4,370	61 Dunblane	2,732
40 St. Andrews	4,311	62 Sanquhar	2,709
41 Dunbar	3,965	63 Dornock	2,680
42 Maybole	3,946	64 Methven	2,653
43 Beith	3,755	65 Linlithgow	2,557
44 Kirkaldy	3,747	66 Nairn	2,504
45 Kirkintilloch	3,740	67 Peebles	2,485
46 Hawick	3,688	68 Selkirk	2,422
47 Clackmanan	3,605	69 Cromarty	2,413
48 Banff	3,603	70 Inverkeithing	2,400
49 Rothsay	3,544	71 Lochmaben	2,392
50 Stornaway	3,500	72 Tain	2,384
51 Thurso	3,462	73 Kinross	2,214
52 Annan	3,341	74 Prestonpans	1,995
53 Crieff	3,300	75 Whithorn	1,935
54 Dumbarton	3,121	76 Stranraer	1,923
55 Girvan	3,097	77 Lauder	1,742
56 Dunse	3,082	78 Kirkwall	1,715
57 Tranent	3,036	79 Wigtown	1,711
58 Forres	2,925	80 Inverary	1,113

N. B. All the towns in Scotland in the preceding list, except Linlithgow, comprehend likewise the country parts of their parishes. This is sometimes the case with those in the table of English towns, but not so frequently.

Article V.

On the Specific Gravity of the Gases. By Thomas Thomson, M.D. F.R.S.

An accurate knowledge of the specific gravity of the gases is of the utmost importance to the progress of chemistry. Their combinations can hardly be made any other way than by measure. Of course all our knowledge of the proportions in which they unite depends upon the accuracy with which their specific gravities have been determined. I have given in my System of Chemistry (Vol. iii. p. 489) a table of the specific gravities of all the gaseous bodies known at the time, formed upon the best authorities that could be procured; but as a great deal has been done on this subject since the publication of the last edition of that work, the table stands in need of numerous corrections, On that account I think it will be useful to publish a new table here. I shall at the same time give the authorities upon which the specific gravities depend.

Table of the Specific Gravities of the Gases.

Names of Gases.	Specific Gravity.	Weight of 100 Cubic Inches.
1 Air	1·0000	30·50grs.
2 Phosgene gas	3·669	111·91
3 Silicated fluoric acid	2·990	91·195
4 Chlorine	2·713	82·75
5 Nitrous acid	2·427	74·0234
6 Euchlorine	2·409	73·474
7 Fluoboracic	2·370	72·31
8 Vapour of ether	2·250	68·625
9 Sulphurous acid	2·193	66·89
10 Vapour of alcohol	2·1	64·227
11 Nitrous oxide	1·614	49·227
12 Carbonic acid	1·518	46·313
13 Muriatic acid	1·278	38·979
14 Sulphureted hydrogen	1·177	35·89
15 Oxygen	1·104	33·672
16 Nitrous gas	1·0388	31·684
17 Olefiant gas	0·974	29·72
18 Azote	0·969	29·56
19 Carbonic oxide	0·956	29·16
20 Hydrophosphoric	0·870	26·53
21 Steam	0·6896	21·033
22 Ammonia	0·590	18·000
23 Carbureted hydrogen	0·555	16·99
24 Arsenical hydrogen	0·529	16·134
25 Phosphureted hydrogen	0·852 0·435	25·986 13·265
26 Hydrogen	0·073	2·230

These specific gravities, as the reader will perceive, are all referred to that of common air, which is reckoned unity. Sir Humphrey Davy has adopted hydrogen as the standard, because it is the lightest; but there are two objections to the adoption of such a standard: 1. Pure air can be obtained every where; and it is, in fact, by comparing the weight of the gas examined with that of an equal bulk of common air that its specific gravity is determined. To adopt hydrogen as a standard would be to add another step to the process by which specific gravities are determined without adding any thing whatever to the precision of the results. Thus the chance of error is increased, without any advantage. 2. It is more difficult to determine the specific gravity of hydrogen gas with accuracy than that of any other gas. It is so light that the presence of the vapour of water makes a sensible alteration in the result. Accordingly that weight varies according to the care with which the gas has been dried. Hence to employ hydrogen as a standard would be not only to subject the result to all the chances of inaccuracy from the experiment itself, but to all those likewise to which hydrogen itself is exposed. The specific gravity of common air can be taken with more ease, and with more accuracy, than that of any other gas whatever: nor have I any doubt that the knowledge of its specific gravity resulting from the experiments of Sir George Shuckburgh is as precise as is wanted for the most delicate investigations.

The weight of 100 cubic inches of each gas, as given in the table, is supposed to have been taken when the thermometer stood at 60°, and the barometer at 30 inches.

I shall now proceed to state the documents according to which the preceding table was drawn up.

1. The weight of 100 cubic inches of dry atmospherical air was determined by Sir George Shuckburgh Evelyn. His method was to ascertain the relative weights of water and air, and then to determine the exact weight of a cubic inch of water. Now a cubic inch of distilled water at the temperature of 60° (when the corrections made upon Sir G. Shuckburgh Evelyn's experiments by Mr. Fletcher [See Nicholson's Journal, Vol. iv. p. 35] are attended to) weighs 252·506 grains: and the weight of air is to that of water at the temperature of 60°, and when the barometer stands at $29\frac{1}{2}$ inches as 0·001188 to 1, according to Sir G. Shuckburgh. Hence if the barometer were at 30 inches (the thermometer remaining at 60°) the weight of air would be to that of water as 0·001208 to 1. From this it is easy to deduce, by the common rule of proportion, that if 100 cubic inches of water weigh 25250·6 grains, 100 cubic inches of air will weigh very nearly 30·5 grains. If we take the French estimate of the weight of a cubic inch of water at 60°, namely, 252·72 grains, the weight of 100 cubic inches of air will turn out nearly 30·53 grains; but as the French experiment was made at the temperature of 40°, and the change upon the bulk of water when heated up to 60° is made by calculation, there is a greater chance of error in such an estimate than in calculating from the experiments of Sir G. Shuckburgh Evelyn, where the heat differed only a few degrees from 60°. On that account I consider his result as the one which chemists ought to adopt.

2. The specific gravity of phosgene gas, as stated in the table, is obtained by adding together the specific gravities of chlorine and carbonic oxide gases; for according to Mr. John Davy it is composed of equal measures of these two gases condensed into half their bulk.

3. The specific gravity of silicated fluoric acid is the result of Mr. John Davy's experiments, given in the Philosophical Transactions for 1812, part 2d. An account of these experiments is given in the present number of our Journal.

4. The specific gravity of chlorine is the result of my own experiments, made with as much care as possible. Other chemists have given it lower. I presume their gas contained a mixture of euchlorine gas. My gas was procured from a mixture of oxide of manganese, common salt, and sulphuric acid, and was passed through water; so that neither muriatic acid gas nor euchloric gas could be mixed with it.

5. Nitrous acid gas is given from the experiments of Sir Humphrey Davy in his *Researches*. He obtained this gas by saturating nitrous gas with oxygen in a glass globe, and knowing the specific gravity of the two gases employed, and the condensation, it was easy to determine the specific gravity of the gas formed. The number in the table would admit of some slight corrections; but as this gas is never united with determinate bulks of others, its specific gravity is of comparatively little consequence.

6. Euchlorine gas was weighed by Sir Humphrey Davy when originally discovered. It was this determination, given in the Philosophical Transactions for 1811, that I have employed.

7. The specific gravity of fluoboracic acid was determined by Mr. John Davy, and the result which he obtained published in the Transactions for 1812, part 2d. We have given an account of his experiments in this third number of our Journal.

8. The specific gravities of the vapour of ether and of alcohol were communicated to me by Mr. Dalton by letter. I do not know the method which he employed to determine them.

9. I have taken the specific gravity of sulphurous acid gas as determined by Sir Humphrey Davy (Phil. Trans. 1812, part 2d) as the latest and most accurate. Kirwan stated it as high as 2·2553; but from his own account of his experiment little reliance could be placed in it.

10. The specific gravity of nitrous oxide is given as it was determined by Sir Humphrey Davy in his *Researches*. Berthollet states it as low as 1·3629; but it is probable that the gas which he examined contained a mixture of common air.

11. I have given the specific gravity of carbonic acid gas according to the experiments of Saussure (Ann. de Chim. vol.

lxxi. p. 262), which seem to have been made with great accuracy. They differ but little from the preceding experiments of Kirwan. Allan and Pepys found the specific gravity of this gas 1·524.

12. I have given the specific gravity of muriatic acid according to the late experiments of Sir Humphrey Davy. They coincide exactly with those of Biot and Arrago. This exact coincidence I consider as a strong proof of accuracy. Mr. Kirwan's previous estimate, founded on calculation, was not entitled to confidence.

13. The specific gravity of sulphureted hydrogen gas is according to the late determination of Sir Humphrey Davy (Phil. Trans. 1812, part 2d). We had two previous determinations: one by Kirwan, that it was 1·106; another by Thenard, that it was 1·236. If this gas be obtained from sulphuret of iron it is always mixed with hydrogen gas. Some such mixture may have occasioned Kirwan's mistake. It is not so easy to account for the error of Thenard.

14. The specific gravity of oxygen gas, as ascertained by Saussure, is in all probability pretty near accuracy. He makes it 1·114 (Ann. de Chim. vol. lxxi. p. 260). Kirwan, Lavoisier, Biot, and Arrago, make the specific gravity of this gas 1·103. Messrs. Allan and Pepys make it 1·090; and Sir Humphrey Davy, in his *Researches*, makes it as high as 1·127. The result of the experiments of Fourcroy, Vauquelin, and Seguin, coincides very nearly with the subsequent experiments of Allan and Pepys. They found it 1·087. The mean of all these experiments is 1·104; and we have little doubt that this result is very near the truth. On that account it has been inserted in the table.

15. The specific gravity of nitrous gas is given according to the results obtained by Berard, and published by Gay Lussac. If Gay Lussac's statement, that it is composed of equal bulks of oxygen and azotic gas without any condensation, be true, then its specific gravity ought to be 1·0375, which differs but little from that given in the table. Sir Humphrey Davy, in his *Researches*, makes it as high as 1·094.

16. The specific gravity of olefiant gas is the result of my own experiments. They have been published in the first volume of the Transactions of the Wernerian Society of Edinburgh.

17. The specific gravity of azote, as given in the table, is the result of the experiments of Biot and Arrago. If we suppose the specific gravity of air 1, of oxygen 1·104, and that air is a mixture of 79 parts by bulk of azote, and 21 of oxygen, then the specific gravity of azote by calculation comes out 0·9723. This does not differ much from the number in the table.

18. The specific gravity of carbonic oxide in the table was found by Mr. Cruickshank, the original discoverer of this gas. I do not know that his experiment has ever been repeated with requisite care. I have myself more than once taken the specific gravity of this gas; but as the specimens which I tried were never perfectly free from carbureted hydrogen, I do not consider my results so accurate as those of Cruickshank.

19. The specific gravity of hydrophosphoric gas was determined by Sir Humphrey Davy when he discovered it, and the result of his experiments published in the Philosophical Transactions, part 2d. We shall give an account of them in our present number.

20. The specific gravity of steam was determined by Trales. I do not know the method which he employed, nor how far the accuracy of his result may be depended on.

21. The specific gravity of ammonia given in the table is that found by Sir Humphrey Davy. As it is the latest, and as he was acquainted with all that had been done by Berthollet and Henry, and as he was at considerable pains, I consider it as likely that his result approaches most nearly to accuracy. Biot and Arrago found the specific gravity of ammonia 0·59669, which coincides very nearly with the number in the table.

22. The specific gravity of carbureted hydrogen as given in the table was determined by myself. The experiments may be found in the first volume of the Memoirs of the Wernerian Society of Edinburgh. Sir Humphrey Davy makes it a little lighter. He found it 0·491. Cruickshank made it 0·67774. Dalton 0·600. My experiments are nearly the mean between all these, and therefore are probably nearest the truth.

23. The specific gravity of arsenical hydrogen gas was ascertained by Trommsdorf.

24. The specific gravity of phosphureted hydrogen gas is not yet determined in a satisfactory manner. I have given two different results. The first was found by Mr. Dalton and Dr. Henry; the second by Sir Humphrey Davy.

25. The specific gravity of hydrogen gas has been very often determined. Sir Humphrey Davy makes it 0·074. The number in the table I consider as approaching as nearly to accuracy as possible, in the present state of our knowledge. It is the result of many trials, at low temperatures; and the quantity of vapour present was calculated according to Dalton's tables, and allowed for.

Upon the whole, I flatter myself that this table, such as it is, will be of considerable service to practical chemists.

Article VI.

Report respecting various Hydraulic Machines, presented to the Imperial Institute of France, by M. Mannoury Dectot.

Paris, Dec. 30, 1812.

The Perpetual Secretary for the Sciences certifies that what follows is an extract of the *proces-verbal* of the sitting of Monday the 28th December, 1812.

M. Mannoury Dectot has submitted to the Class several hydraulic machines of his own invention, respecting which M. de Prony, M. Perier, and myself, were charged with making a report.

We will acknowledge that ten new machines proposed all at once by the same person, who had likewise announced several more which were soon to follow the former, filled us at first with some suspicion, knowing how much trouble and labour it costs even the most ingenious artists to invent a single machine, possessed of some remarkable or useful properties.

But this suspicion was unjust, and was soon succeeded by that pleasure which we always feel on discovering a mind as exact in its assertions as fertile in its resources.

The principles of mechanics in general, and of hydrodynamics in particular, have been long known. Every body must be aware that what is called an invention in them can be only a new combination of general principles. We know further that in hydrodynamics especially, the subject is so complicated for the calculus, that it is generally impossible to know *a priori* what will be precisely the effect of such or such a combination; so that experiment is absolutely necessary to confirm or destroy those results which seemed to follow from theory.

M. Mannoury has neither spared trouble nor expense to remove in this respect all doubts. The commissioners have been witnesses of his numerous experiments, and in consequence have been able to give that sanction to his discoveries which can alone place them in the number of certain and positive facts.

The general problem which M. Mannoury proposed to himself is this: *a fall of water being given, to elevate a portion of that fluid above the reservoir, by means of a machine all the parts of which are absolutely fixed;* and which of course neither contains levers, nor wheels, nor pistons, nor valves, nor any other moveable part.

One is much inclined at first to consider this problem as impossible, and we do not know that it has been undertaken, or at least solved, by any person. In fact, a reflection immediately presents itself, which seems to destroy all hope; namely, that if such a machine were possible, it would probably occur accidentally, among the numerous combinations of fixed matter which nature offers to our view. From such combinations it might follow, for example, that a reservoir of water formed on the shoulder or in the interior of a mountain, by means of rain water, might produce a jet of water upon the summit of the mountain—an effect which appears chimerical, because at the sight of a source of water at the top of a hill, we conclude in general immediately that this source comes by means of subterraneous currents from some mountain in the neighbourhood of a higher elevation.

Yet M. Mannoury has solved this problem in various ways, quite unconnected with each other. Facts answer all objections; and the theory, though it cannot always foresee the truth, serves at least to confirm it, and to generalise individual facts.

Notwithstanding the surprising variety of the machines proposed by M. Mannoury, and the very complicated nature of some of them, yet, if we compare them with care, we shall find that they consist of various combinations of three principal methods, employed either together or separately. These methods are distinguished by M. Mannoury by the three following names: *the intermitting syphon*, *the hydreole*, and *the oscillating column*. We shall satisfy ourselves with describing here in what each of these three methods consists, without entering into the detail of all the applications of them that the author has made, which would be very tedious, and would require the description of machines too complicated to be understood without figures.*

INTERMITTING SYPHON.

The intermitting syphon is not a thing unknown in physics; for it is by its means that those fountains, called *reciprocating*, are emptied each time that their reservoir is found filled with rain water, or any other water, up to a height above the top of the syphon. Then the water begins to run by the longest branch, and the water continues to run out till its surface comes to a level with the mouth of the shortest branch; but the novelty in the mechanism of M. Mannoury is the use to which he has put this intermitting syphon, so as to make it the principle upon which various machines are constructed, which have none of their parts moveable, and notwithstanding elevate water above the reservoir.

To conceive this intermitting effect, we have only to suppose it applied to the ordinary fountain of compression, called the

* Considering the great number of machines presented by M. Mannoury Dectot, M. M. the Commissioners of the Institute have thought it requisite only to make known his general principles. The author will speedily publish his memoir, in which he has described all his machines with care. He will establish their theory from a series of comparative experiments, from which he will deduce practical instructions.

fountain of Hiero. It is well known that this fountain of Hiero is composed of two close compartments, placed the one above the other, and separated by a diaphragm. By introducing water into the inferior compartment, by means of a pipe from the reservoir, the air whose place it occupies is gradually compressed. It is compressed likewise in the superior compartment, in consequence of a tube of communication established between the two compartments. The water in the superior compartment being thus pressed upon, rushes through a pipe, and ascends above the reservoir.

But this effect, being produced only by the compression of the air in consequence of the introduction of water into the lower compartment, ceases as soon as this compartment is filled with water, because then the air ceases to be compressed in the upper compartment. To renew the effect we must empty the lower compartment of the whole of its water. This is what M. Mannoury accomplishes by means of his intermitting syphon. This syphon being fitted to the inferior compartment, empties it at once, as soon as the water reaches the top of the syphon. Air rushes in, and supplies its place, and things are brought back to the situation in which they were before the machine began to play. The jet is again renewed by the falling of the water, and thus the machine continues to raise water above the level of the reservoir without having any moveable part whatever. We have only to repeat this mechanism by a suite of similar fountains placed in stages below one another, to raise water to any height we please by means of a loss proportional to what runs through the intermitting syphons. This is what M. Mannoury does in one of his machines, which is nothing but a collection of various fountains of compression, connected with each other, and so contrived that all are brought into action by means of one intermitting syphon adapted to the inferior compartment of the lowest of the fountains.

It is easy to see that the intermitting syphon may be applied likewise to various other machines to restore their energy after they have produced a given effect, and thus to enable them to continue their action. M. Mannoury varies his applications; but the example which we have given is sufficient for explaining the way in which this first mode of raising water above the reservoir may be applied to machines without any moveable parts.

HYDREOLE.

The author has given the name of *hydreole* (composed of ὕδωρ, water, and Αιολος, Æolus) to the machines in which he employs a mixture of water and air to make the first of these fluids ascend above its natural level. This method consists in putting two columns in equilibrio, the one of pure water, the other of water mixed with air. The last mixture, having a smaller specific gravity than the first, can only counterbalance it by means of a greater height. Hence it follows that the mixed column ought to rise above the reservoir, and of course carry the water which it contains above its natural level.

This effect is not unknown to philosophers. It constitutes the principle of the pump of Seville; and M. Cagnard-Latour has already applied it to a machine in which he produces the mixture of water and air by means of Archimedes's screw reversed; but if the principle is not quite new, there is at least much novelty in the manner in which M. Mannoury makes his mixture so as to be very intimate. He is not satisfied with introducing a volume of air into a volume of water. He wishes the air in the first place to be divided into a number of minute bubbles, which being lodged among the molecules of the water, should be kept separate from each other, and retained by adhesion in such a manner that they are only disengaged slowly, and do not unite in order to escape till the object wanted by their presence has been attained.

M. Mannoury distinguishes two sorts of hydreoles; the hydreole by suction, and the hydreole by pressure.

When a column of water moves in air, it drags a quantity of the elastic fluid with it, either in consequence of an adhesion between the two, or because a species of vacuum is formed round the column of water towards which the surrounding air rushes. This last effect is demonstrated by the fine experiments of Venturi. It follows from this that water, in passing through a mass of air, absorbs a part of it, and becomes in some measure gaseous; and this is what M. Mannoury calls a hydreole by suction.

If on the contrary we suppose that a volume of air is driven by force into a mass of water, by bellows, or any other means; and that this volume of air, in penetrating into the water, is divided into a great number of small bubbles, by being made to pass through very small holes, the mixture that results is what M. Mannoury calls a hydreole by pressure; because in reality it is by a strong compression of the air that it is forced to enter and mix with the water.

In both of these hydreoles the water thus mixed with air becomes lighter than pure water, and of course capable of mounting higher than the reservoir. Such is the basis of the second method proposed by M. Mannoury. The author, as usual, varies his applications. It will be sufficient here to point out one or two.

Let us conceive a reservoir to the bottom of which is adapted a bent tube, one branch of which rises higher than the reservoir, In its natural state the water will rise to the same level in the tube and in the reservoir.

Let us suppose now that towards the middle of the length of the tube a hole is made, and a pair of bellows adapted to it, by means of which air is forced in, not in a full current, by the interposition of a plate of metal pierced by a great number of small holes to divide the volume of the air. The air will penetrate into the mass of water in the form of small bubbles, and the adhesion of the particles of water to one another will keep these small bubbles separate. The water of the tube will thus become mixed with air, above the opening at which the pipe of the bellows is fixed, and of course specifically lighter than the water of the reservoir. It will therefore stand higher in the tube than in the reservoir, and may be made either to return into the same reservoir, or it may pass into another more elevated than the first; but the object of M. Mannoury would not be completely fulfilled, unless he substituted for the bellows, which are moveable, some other method. His contrivance is very simple.

The author makes a second column pass from his reservoir which makes its way through a new tube into a close chamber. In proportion as the water fills this chamber the air in it is compressed. This compressed air passing by means of a tube to the opening in the side of the first tube answers the purpose of a bellows till the chamber becomes filled with water. The chamber is then emptied by means of an intermitting syphon, and the same processes are repeated at pleasure.

The author, to render his current of air continual, has contrived very ingenious methods, of which it is not necessary to speak here. It is only necessary to say that the effects of this hydreole correspond exactly with the promises of the author, and that a constant and copious supply of water is raised by it.

We shall notice another application of the hydreole, on account of its singularity.

The author begins by drawing from his reservoir a jet d'eau, which rises, according to the common laws of hydraulics, not quite so high as the reservoir, on account of the friction. At the centre of the pipe from which this jet d'eau issues, a current of air rushes, produced, as above explained, by means of a second column of water from the same reservoir. The water and the air, in consequence of this, mix together, and issue in that state from the mouth of the pipe, and rise in consequence much higher than the reservoir. This effect ought to be expected from what has been said before; but what is very singular, is the noise occasioned by the shock of the particles of air against those of the water as they issue from the pipe. This sound approaches that of the harmonica; but it is not so sweet. If the running of the water be stopped, the air which issues alone occasions only a slight hissing noise.

OSCILLATING COLUMN.

The third method thought of by the author, for raising the water of a reservoir above its natural level, is what he calls *the oscillating column.* Of his three principal methods, it is the one which appears to us the newest. We know nothing that could have suggested the fundamental idea of it. It is exceedingly simple, since it consists of nothing but a bent tube, adapted to a reservoir, the continuity of which is interrupted towards its lowest part. It is this interruption of continuity in the tube which occasions, what we see with surprise, the water to mount above the reservoir, without the addition of any other piece to the machine.

To explain this phenomenon, let us suppose a syphon reversed, that is to say, with the open ends of the branches turned uppermost. If we drop a ball into one of these branches without communicating to it any momentum, it is evident that in consequence of the velocity which it will acquire by the fall, it will ascend in the other branch to the same height from which it fell in the first, and that, abstracting the effect of friction, this ball will continue to oscillate in the syphon rising in both branches to the same height.

But if we make a second ball succeed the first, and in contiguity with it, then the first ball will rise in the second branch of the syphon to a greater height than that from which it descended, and the second to a less height; for it is the centre of gravity of their system, that is to say, their point of contiguity, which ought to ascend to the point from which they set out. The same thing would hold if there were a greater number of balls.

This shows us that water poured into one of the branches of the syphon ought to rise higher in the other when we continue to pour it into the first: however, as it cannot rise indefinitely in the second branch, a time arrives when it begins to press upon the column below, and to push it back into the first branch. This effect may be easily estimated by the principle of the *conservation of living forces;* for it results from that principle that the moment the column becomes stationary, in order to flow back, the centre of gravity of the mass ought to be precisely at the height of the open extremity of the first branch of the syphon; since it is by it that all the water has been introduced, and that it is supposed to have no other velocity but what it acquired by flowing from that extremity.

But according to the same principle, if at the moment when the water becomes stationary, one were to subtract or annihilate the small portion of fluid which fills the lowest part of the syphon, that is to say, in the bending of the syphon where it is horizontal, this portion of fluid being animated with no *living force*, either actual or potential, the sum of the living forces of the whole mass would not be altered, but it will be distributed through a smaller mass.

Suppose we continue to pour water into the first branch of the syphon: this new quantity of fluid will bring a new sum of living forces to the mass. If then at each oscillation we were to remove a portion of inactive fluid, while on the other hand we introduce a new change of living forces, the sum total of the living forces would continually increase, though the total mass remained the same. Hence the ascent of the water in the second branch of the syphon would increase indefinitely.

But if we wish to set limits to this increase of the living force of the column, we have only to cut off the second branch of the syphon at any height we think proper, then at each oscillation a portion of fluid will flow from the end of that branch, and thus the water will be made to rise higher than the reservoir.

The difficulty then is reduced to abstract the competent portion of fluid which fills the lower part of the syphon the moment the fluid becomes stationary, and this without employing valves or any moveable piece of machinery whatever. This M. Mannoury accomplishes in a very simple manner, by establishing, at the lower part of the syphon, a small solution of continuity between the two branches of the same syphon.

When the water is animated by a rapid movement of oscillation it cannot escape through that opening, because its acquired motion enables it to clear the small interval; but the moment it becomes stationary, the acquired motion having now no place, the portion of fluid, which corresponds to the small opening, escapes; and this is precisely what must happen in order that the machine may be able to produce its effect, without any loss of living forces, as we have explained above.

Still more certainly to prevent the escape of water, before it has acquired a state of rest, M. Mannoury makes the lower end of the first branch of his syphon terminate in a truncated cone, which occasions a contraction in the fluid vein that issues out of it, and determines it to enter entirely the second branch.

It is much easier to explain than to foresee this singular effect; but success has justified this delicate experiment of M. Mannoury. It is obvious that much time and many trials are necessary to produce from this principle an useful machine. At present we can only consider it as a very curious experiment.

M. Mannoury wished to know what would happen when the second branch of the syphon was shut at its upper extremity by means of a plate, leaving only a small opening in that plate. What happened was as follows... The column of water, which mounts by its oscillations in the second branch of the syphon, finding itself all on a sudden stopped by the plate at the top, produces the ordinary blow of the hydraulic ram. The living force is in part destroyed by the shock, and the rest of it passes in a jet of water which corresponds to the small opening in the plate, and this jet is forced to a great height. This effect, which is common to the machine of Montgolfier, and of M. Mannoury, does not prevent the two machines from differing essentially from each other. Montgolfier's machine cannot dispense with its valves, which are essential to it; while the oscillating column of M. Mannoury has no valve whatever, and preserves the fundamental property of all the machines which he has submitted to the judgment of the Class. By the combination of so many methods, either little known or altogether unknown, in the construction of his hydraulic machines, M. Mannoury has deviated from the ordinary circle of ideas, according to which such machines are conceived; of course it was likely that he should arrive at results quite unexpected. The author unites the knowledge acquired by study with that fineness of tact which produces inventions. It is therefore to be presumed, that in his hands several of his machines, which at present are merely curious, will acquire a perfection which will make them still more interesting on account of their utility.

The intermitting syphon, and the hydreole, give already very good results, as far as they can be judged of; but new experiments are wanting to measure exactly their products.

We shall defer till another time the particular report which we propose to make of the corn-mill invented by M. Mannoury. We shall only say that this new mill seems to be of more public utility than any other. There are already 14 established at the forges of Paimpont in Britanny, and the departments of the Orne, of the Manche, and of Calvados. It consists in a happy application of the *machine à reaction*, contrived by M. Segner, of the Academy of Berlin, and afterwards subjected to calculation by several illustrious mathematicians, particularly by the two Eulers, father and son, and by M. Bossut. According to very accurate and well-authenticated experiments, these mills produce an effect superior to the best executed ordinary mills.

The commissioners are of opinion that M. Mannoury has rendered essential service to the theory, as well as the practice, of the motion of water, by his researches and his experiments; and that his inventions deserve the approbation of the Class.

(Signed) DE PRONY, PERIER, CARNOT, Reporter.

The Class approves the Report, and adopts the conclusions contained in it.

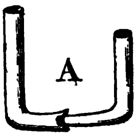
Certified as conformable with the original,

The Perpetual Secretary, Chevalier de l'Empire,

(Signed) DELAMBRE.

Note by the Editor.—What is called *vis viva,* and translated in the preceding paper *living force,* is a mode of expression much used by the philosophers on the Continent, though not familiar in Britain. It means nothing more than that the square of the velocity multiplied into the bodies continues unaltered before and after the shock, oscillation, &c. to which the bodies are subjected.

The following explanation of the oscillating column suggested by Sir Charles Blagden appears to me much more intelligible than the one in the text:—



Let A be the interrupted syphon. Suppose both legs full of water up to a certain height, and that we continue to pour water into the short leg. Part of that water will spend the whole of its impetus in striking against the water in the long leg, and then escape at the interruption of contiguity; so that in fact the water in the longer leg will be exposed to a greater pressure than it otherwise would be, and must of course rise higher.

Article VII.

On Formations. By Robert Jameson, Esq. F. R.S. E. Professor of Natural History, and Keeper of the Museum in the University of Edinburgh, &c.

On a general view of the materials and structure of the crust of the earth, we are struck with the simplicity of the whole. Not more than 250 species of simple minerals have hitherto been discovered; and if we abstract the metalliferous, saline, and inflammable species, there remain not more than 134 species of earthy minerals. Still, with this small number of species, nature might have formed many hundred distinct, compound, and simple rocks; but it is otherwise. She employs almost exclusively a few species in the composition of all the rocks, both simple and compound, of which the crust of the earth is composed. Felspar, quartz, mica, minerals of the hornblende family, and limestone, are the most frequent and abundant: of these nearly the whole crust of the earth is composed: thus granite, gneiss, mica slate, clay slate, gabbro, porphyry, sienite, greenstone, basalt, serpentine, sandstone, are composed of one or more of the four first mentioned substances; and the various, primitive, transition, and floetz limestones, that often form extensive ranges of mountain and tracts of country, are composed of carbonate of lime. Indeed, all the species of mountain rocks, at present known, do not exceed 50 or 60; and amongst these are several enumerated in the list of simple earthy minerals. Some mineralogists have considered them as more numerous; and have described every variety of composition as a distinct species, and in place of 50 or 60 species have enumerated several hundred. But the greater number of these are mere varieties of the common mountain rocks, of limited extent, often not exceeding a few fathoms in length and breadth. This error is owing to a misconception of what is understood by a mountain rock. A mountain rock is distinguished by its position in regard to other rocks, its magnitude, under which I include its length, breadth, and thickness, in the tract where it occurs, and the proportion and kind of minerals of which it is composed. Viewing them in this manner, it is not sufficient that they exhibit varieties in structure or composition, they must also have a determinate position and considerable magnitude, and the certainty of the distinction is augmented if they possess universality of distribution. Most of the mountain rocks are universally distributed: thus the compound rock, known under the name granite, and which is so abundant in Europe, occurs in China and Van Diemen's Land, at the Cape of Good Hope and in Bengal, in Brazil, Mexico, and Canada; and in all these countries it has the same characters. From this fact it follows that we can with confidence draw inferences in regard to the geognostic characters of rocks in one country from what has been observed in another, and consider these as applicable, on a general view, to the whole crust of the earth. Experience does not contradict this inference; on the contrary, it is confirmed by the investigations of geognosts in the most distant countries. This universality of the distribution of formations, consequently of the laws of the formation of the earth, has procured for geognosy a place amongst the physical sciences; and in it lies that which leads so irresistibly to geognostic investigations, as soon as we begin to occupy ourselves with the study of this branch of knowledge. It would wear out the patience of the most persevering inquirer, and would convey no very satisfactory information of a new set of rocks, or a new arrangement, if those already known were to be studied in every small tract of country. We might as well pretend to arrange and number the grains of sand on the sea shore. In every country of considerable extent we find the outline of the structure of the whole globe.

Some series of rocks, however, do not possess this universality; they appear in bason-shaped hollows, or in countries that have been formerly in the state of lakes, or in level plains resting on alluvial matters: their extent is, comparatively, inconsiderable;

and all the phenomena they present attest the partiality of their deposition. We cannot, from these appearances, infer any thing in regard to the general structure of the earth; and however interesting they may appear to us, it would lead to erroneous views were we to deduce from them general inferences in regard to the structure of the earth; for every general inference of this kind from a local appearance must be false. These series of rocks, to distinguish them from the more widely distributed or universal, are denominated *partial or local formations*. This interesting division was first pointed out by Werner. He was led to it by the examination of a series of rocks at Wehrau, in Lusatia. To the common observer these rocks might have passed for members of the universal series; but his judgment and penetration led him to ascertain that their characters were of such a nature as to afford proofs of the existence of a kind of formation of solid rocks hitherto unsuspected. The idea was not lost with him; for he inferred that such formations would be found in other similar situations, and that the bottoms of lakes, the sides of rivers, &c. would frequently present appearances of this kind. These local formations are less striking in low and flat countries than in mountainous regions—where they are contained in valleys and their boundaries strongly marked. Hence we must be careful, in describing the rocks of low and flat countries, not to confound partial or local with universal formations.

The celebrated Von Buch, in one of the late numbers of the magazine published by the Society of the Friends of Natural History in Berlin, describes an uncommonly interesting local formation which he discovered at Locle, in the district of Jura. It is contained in a high enclosed valley, situated 1665 Fr. feet above the level of the lake of Neufchatel, and 2959 Fr. feet above the level of the sea. The valley, and the strata it contains, are about two miles and a quarter long, and about a mile broad. It is surrounded with high mountains of white compact limestone; and its bottom is of the same species of rock. It is completely inclosed; and the water that falls in it escapes by subterraneous unknown canals. These canals may have opened for the first time not many centuries ago, before which period the whole valley of Locle must have been a lake. Even still the canals are so narrow that the valley is frequently overflowed: this circumstance induced the inhabitants in the year 1802 to cut long levels into the sides of the bounding mountains, in order to permit the water to escape into the lower valleys. This undertaking succeeded completely. The valley contains many small hills, from 200 to 300 feet high. The lowest stratum of these hills, which rests immediately on the limestone, is a very coarse conglomerate of masses of the neighbouring limestone. On it rests a pretty thick bed of marly limestone, which has a white colour, is fine, earthy, and almost friable. Throughout its whole extent it is intermixed with small *river shells,* which still retain their natural shell. Small reeds also occur every where in this bed. It is the most characteristic and extensive rock of the whole formation. In the middle of it we meet with beds of smoke-grey *hornstone,* which has a fine splintery, or imperfect conchoidal fracture. These beds are the more remarkable, from the circumstance of siliceous beds occurring very rarely in the limestone of Jura; and when they do appear, are never so pure and distinct as in the partial formation of Locle. The same species of fresh water shells as occur in the marly limestone, also appear in the hornstone: amongst these can be distinguished the Helix cornua, a species frequent in the Lower Rhine, but which has not hitherto been found in Switzerland. Below the hornstone lies a bed of *opal,* which is of a brownish black colour, glistening lustre, and perfect conchoidal fracture. This, Von Buch observes, is a remarkable production to be formed in the water of a lake; and is, in his opinion, a hornstone coloured with the coally matter of decayed vegetables. To this opal succeeds a bed of *bituminous shale,* which contains many impressions of reeds; and next, a bed of *coal,* including numerous bivalve shells. This coal burns badly; yet it is used by blacksmiths, when a strong fire is required. These beds are in general but a few inches thick, but alternate two or three times as we descend; and it is said they sometimes attain the thickness of two feet. The whole of these minerals are the produce of a small inclosed lake; for not a trace of these rocks is to be seen beyond the mountains that surround Locle. We thus see what lakes have produced · hills of 300 feet in height, and compact siliceous beds which are almost crystalized. Indeed, crystals of quartz sometimes occur in the fissure of the hornstone.

Another very curious local formation occurs at Æningen, on the Rhine. The remarkable limestone rocks of that spot have long engaged the particular attention of mineralogists. The celebrated quarries of Æningen were said to contain organic remains from every quarter of the globe, and in them it was supposed we could read the whole history of the earth. The acute and learned Blumenbach, however, after examining this formation, conjectured that it was of very new date; that it was formed by one of those partial local revolutions which, according to him, have taken place since what he calls the last general catastrophe which our earth has experienced. Von Buch is of opinion that it is a local formation, a deposition which had taken place in a previously existing lake from the rivers and rivulets having deposited slime from the adjacent country over fishes, insects, leaves, and other organic bodies, and gradually enveloped them in thin beds of mud: probably long after these places were inhabited; probably even after the erection of the neighbouring churches and cloisters. Similar depositions take place at this day in limestone countries where calcareous tuff is formed; and it is well known that in the Travertine rock of Rome, a formation of the latest period, we find leaves, &c. of the various trees which now grow in the adjacent country. This very probable opinion of Von Buch's is founded on the excellent description of Æningen, published by Dr. Karg, of Constance, which contains the first accurate view of the country of Æningen.

The petrifactions are contained in a slaty limestone of loose texture. It extends about a mile in length, and fills a hollow in the limestone rocks, and not a trace of it is to be seen in the neighbouring country. The valley appears to have emptied itself at no very remote period, and left exposed at its bottom the limestone slate of Æningen.

Dr. Karg gives an accurate and interesting systematic catalogue of all the petrifactions hitherto found in this limestone, and shows how observers have been deceived, particularly when they imagined that they had before them American and Indian, even entirely unknown organic forms; and declares, after a careful and accurate examination of many hundred petrifactions, that he is not inclined to consider any of them as exotic. Indeed, we cannot but consider this opinion as well founded, when we attend to the many remarkable histories given by Dr. Karg of Æningen petrifactions. Thus Scheuchzer's Homo diluvii testis which probably lived at a later period than the building of the neighbouring cloisters of Petershausen, even during Scheuchzer's life-time, was by himself admitted to be but a quadruped. An exotic porpoise, under the hands of Dr. Karg, proved to be the common pole-cat; and the shoots and leaves of the vine, which Walch describes as occurring in this limestone, prove to be nothing more than branches of the black poplar. Among the great number of bivalve shells that occur in the slate of Æningen, Dr. Karg did not find a single species of marine origin; all were of fresh water growth. He also found that all the roots, woods, and leaves, that are inclosed in this rock, belong to some of the vegetable species that now grow in the vicinity. He found very distinct specimens of the branches, leaves, and nuts of the walnut tree (Juglans Regia). But it is said that the walnut tree was imported from Armenia into Italy, and from thence distributed over Germany. This interesting fact, Von Buch remarks, leads us very near to the period when the Æningen petrifactions took place, and renders it probable that the formation is of very recent date.

What then can we deduce from the emptying of a lake, and the operations that took place at its bottom, in regard to the structure of the earth and its history? We learn nothing more than what took place where the lake was situated.

Even supposing the lake to have been of considerable extent, still what took place within it could not afford us general laws, such as we obtain by considering the universal formations, as clay slate, grey wacke, gneiss, mica slate, &c.

The formation of Æningen, as Von Buch well remarks, affords a most striking example of the necessity and importance of distinguishing *general* from *partial* or *local* formations. Had naturalists known that the limestone slate of Æningen was a local formation, we should not have had so many erroneous views and absurd inferences drawn from the petrifactions it contains.

Another set of formations, which of late has excited much attention, shall next be noticed. It is the series of new floetz rocks around Paris, and which is by some naturalists, although probably incorrectly, conjectured to be a partial or local deposite. When I first turned my attention to the descriptions of this tract of country contained in the Continental Journals, I was led to conclude that it differed from any of those contained in the arrangement of Werner, and stated it, as my opinion, that it appeared to be of comparatively recent origin. This inference, the truth of which has been demonstrated by the observations of Cuvier and Brongniart, I was enabled to make by applying the principles of the Wernerian geognosy to the accounts that had been published. From these it appeared that this tract of country was composed of alternate beds of sand, clay, marl, earthy soft limestone, sandstone, and gypsum; in which were contained numerous petrifactions of quadrupeds, birds, and other organic remains. Now as Werner has ascertained that the older formations are compact and solid, the newer in general loose and earthy; further, that remains of quadrupeds and birds occur only in the newer formations; I concluded, from the looseness of the texture of the Parisian strata, and their containing remains of quadrupeds and birds, that very probably they belonged to a new formation, or formations, more ancient than the oldest alluvial deposite, but newer than chalk.

It would appear from the late observations of Cuvier and Brongniart in their "Essai sur La Geographie Mineralogique des Environs de Paris," that the rocks of these formations are deposited in a hollow or bason of chalk, which forms the *fundamental rock*, or immediate basis of the district. These formations, according to the French naturalists, are 11 in number, viz.:— 1. Chalk; 2. Plastic clay; 3. Coarse limestone; 4. Siliceous limestone; 5. Gypsum, of the first fresh water formation; 6. Marine marl; 7. Sand and sandstone, without shells; 8. Sandstone, of marine formation; 9. Millstone, without shells; 10. Second fresh water formation of marl and millstone; 11. Alluvial.

1. *Chalk.*—The chalk, which is the oldest and lowest member of the series, contains a considerable number of petrifactions: of these the most characteristic is the belemnite: 22 species of petrifactions are enumerated, but not one of these has been discovered in the superincumbent formations.

2. *Plastic clay.*—This clay varies in purity: the lower bed is the purest, and contains no petrifactions: the upper bed, that next the limestone, is sandy, of a blackish colour, and sometimes contains organic remains. It varies in thickness, from 17 yards to a few inches. It is distinctly separated from the chalk. There is no transition from the one into the other; on the contrary, the clay contains fragments of the chalk, a proof that the chalk must have been consolidated before the clay.

3. *Coarse limestone, and its marine shell sandstone.* —This rock rests on the clay; but it does not every where immediately rest on it, being sometimes séparated by a bed of sand, varying in thickness. The lower bed of limestone is mixed with sand; sometimes, indeed, contains more sand than lime. This limestone formation is composed of alternate beds of coarse limestone more or less pure, clay marl, very thin slaty close marl, and calcareous marl, all arranged in a regular and determinate order. It is filled with petrifactions: of these upwards of 600 have been already described by La Marsk and others. The lowest beds contain petrifactions that do not occur in the middle beds: and in the middle beds we meet with petrifactions that are wanting in the upper beds. It also appears that the number of petrifactions diminish as we approach to the uppermost beds, when they entirely disappear. The middle and upper beds of limestone contain beds of sandstone and hornstone filled with marine shells; and the sandstone sometimes contains both fresh water and marine shells.

4. *Siliceous limestone.*—This formation is composed of strata of limestone, penetrated with silica. It is often cavernous, and the cavities are lined in some instances with siliceous stalactites and crystals of quartz. It contains no organic remains. In this formation occurs one variety of the mineral called *buhrstone*, used for millstones. The authors of the essay are of opinion that the buhrstone is the siliceous skeleton of a limestone: the quartz being deprived by some unknown cause of its lime, there remains now a porous mass, very hard, and containing in its cavities a clay marl.

5 & 6. *Gypsum of the first fresh water formation, and marine marl.*—The gypsum rests on the siliceous limestone. The formation, however, is not entirely gypseous; but consists of alternate beds of that mineral and of calcareous and argillaceous marls. We have an excellent example of this formation at

Mont Martre. There we observe three masses of gypsum. *First Mass.* Rests on the limestone. The lowest part is composed of alternate thin beds of gypsum, including crystals of selenite, and solid calcareous marl, and very thin slaty clay marl. In the gypsum large lenticular crystals of gypsum occur, and in the marl menelite. The gypsum contains sometimes fresh water, sometimes marine shells.—*Second Mass.* In this mass the strata of gypsum are thicker than in the preceding, and the beds of marl less numerous. In the clay marl petrified fishes occur, and also masses of sulphate of strontian.—*Third Mass.* This is the thickest of the three masses, being in some places 30 metres thick. It contains but few beds of marl. The lowest strata of this mass contain silex impregnated with the gypseous matter. The intermediate strata are divided into large columns. The upper beds are penetrated with marl, and also alternate with it. It contains in general five beds of marl. It is in this third mass that the remains of unknown quadrupeds and birds are found. To the north of Paris these petrifactions occur in the gypsum; but to the south, often in the beds of marl that alternate with the gypsum. This gypsum also contains bones of the tortoise, and skeletons of fish, and also fresh water shells. This third mass is essentially characterised by the presence of the skeletons of quadrupeds. These remains serve to determine it when it occurs isolated, for no such remains have hitherto been found in the lower masses. Above the gypsum occur thick beds of calcareous and argillaceous marls. It is in the calcareous marl that we meet with trunks of palm-trees penetrated with silica. In the same beds there occur shells of the genera planorbis and limneus, that scarcely differ from those that live in our marshes. These petrifactions are alleged to prove that these marls are of fresh water origin, like the gypsum on which they rest; and it is remarked that the gypsum, the beds of marl that occur in it, and those that cover it, constitute the first or oldest fresh water formation of the Parisian series of rocks. Above these marls we observe numerous and often thick beds of argillaceous and calcareous marls. They contain no petrifaction, and the formation to which they belong has not been determined. Above these we meet with a yellowish slaty marl, which towards its lower part contains balls of sulphate of strontian, and a little above a thin bed of small bivalve shells belonging to the genus cytherea. It is said that it serves as the limit of the fresh water formation, and marks the beginning of a new marine formation. In short, all the shells we find above it are marine. It is about a metre thick, and contains in its upper layers also cerites, spirobes, and bones of fish. Over this rests a thick bed of green marl. It contains no petrifactions, but nodula of sulphate of strontian. Four or five beds of marl succeed the green marl, and appear to contain no petrifactions; but these beds are covered with a bed of yellowish argillaceous marl, which is filled with fragments of sea shells of the genera cerita, trochus, mactra, venus, cardium, &c.; also fragments of the palate of two species of ray. The beds of marl that succeed these contain principally bivalve marine shells; and in the latter beds, those immediately under the argillaceous sand, contain two beds of oysters. These oysters appear to have lived in the place where we now find them, for they are united together as in the sea; the greater number are quite entire; and if we extract them with care, we find that the greater number have both valves. The gypsum formation is often terminated by a mass, more or less thick, of argillaceous sand without shells.

7. *Sand and sandstone without shells.*—The sandstone without shells is one of the last formations. It constantly covers those already described.

8. *Upper marine sand and sandstone.*—This is termed the last marine formation of the series, and covers the preceding rocks. The sandstone varies in colour, being sometimes grey, sometimes red. It contains marine shells; and these are sometimes different from those of the lower marine formation. It thus appears that there are in the vicinity of Paris three sorts of sandstone, sometimes very similar to each other in mineralogical characters, but differing in their geognostical position. The first, or lowest, makes part of the beds in the coarse or marine limestone, and contains marine shells: the second rests on the gypsum formation; and even the marine marl that covers it is the most extensive, but contains no shells: and the third is only covered by what is termed the last fresh water formation, and immediately follows the second. It is the least frequent of the three, and like the first contains many marine shells.

9. *Mill or buhrstone formation without shells.*—This formation consists of feruginous argillaceous sand, clay marl, and millstone. Thus these substances do not appear to follow any determinate order in their superposition. The millstone is a quartz, containing numerous irregular cavities that do not communicate with one another, and which are traversed by siliceous threads, disposed somewhat like the reticulated structure of a bone, and lined with a crust of red ochre. These cavities are sometimes filled with clay marl, or sand: they are never lined with siliceous incrustations, like calcedony, nor with crystals of quartz. These last characters, independently of its position, are sufficient to distinguish it from the millstone beds of the siliceous limestone formation already mentioned. Another geognostic character of this rock is the want of petrifactions.

10. *Second fresh water formation.*—This formation rests on that last described, and is composed of two kinds of rock, the

one siliceous, the other calcareous. The siliceous mineral is sometimes like flint, sometimes like jasper, and at times it is vesicular, like buhrstone. The limestone is sometimes compact, sometimes marly; often contains irregular cylindric cavities, nearly parallel, though crooked. They resemble exactly the cavities that would be formed in a bed of mud by bubbles of gas rising from the bottom to the surface. This limestone, when fresh gathered from the quarry, has often the property of disintegrating by the influence of the air and water, and hence is used as marl. But a principal character of this formation is the presence of fresh water shells: these are different species of helix, planorbro, limneus, potamides, cyclostoma, gyrogonites, and bulimus.

11. *Alluvial.*—The alluvial, or uppermost formation, is composed of variously coloured sand, marl, clay, or a mixture of these substances impregnated with carbon, which gives the mixture a brown or black colour. It contains rolled stones of different kinds, but is most particularly characterised by containing the remains of large organic bodies. It is in this formation that we find large trunks of trees, bones of elephants, also of oxen, rein-deer, and other large mammalia. This alluvial matter is deposited in hollows that have been scooped out of the solid rocks we have just enumerated. It is a very old deposite, as it appears to have been formed before the commencement of our history, because it contains remains of trees and animals different from any that exist at present in the neighbouring country, or in the globe.

From the preceding account it would appear that the strata around Paris are of clay, gravel, sand, sandstone, millstone or buhrstone, marl, limestone, chalk, and gypsum; and these are said to constitute 11 different formations. It would probably simplify our view of this tract of country, and be equally correct, if we diminished the number of formations in the following manner:—

1. *Chalk formation.* 2. *Coarse marine limestone formation,* under which we would include not only the coarse limestone, but also the siliceous limestone, plastic clay, and sand, because this latter is intermixed with the limestone, and there is an uninterrupted transition from the one into the other. 3. *Gypsum formation,* or first fresh water formation. 4. *Sandstone formation,* under which might be included the sand and sandstone without shells, the upper marine sandstone, and the buhrstone without shells. 5. *Second fresh water formation,* composed of limestone and flint. 6. *Alluvial formation.*

From the intermixture of fresh and salt water, organic productions in these formations, we may suppose that both these fluids must have contributed each their part in their formation.

Cuvier is of opinion that the first two formations, viz. the chalk and limestone, are of marine origin, because they contain principally sea shells; but the limestone contained also many fresh water shells. The third formation, the gypsum, from its containing remains of land quadrupeds, birds, and fresh water shells, is conjectured to have been deposited from fresh water; but it also contains marine shells. The fourth, or sandstone formation, from its containing principally marine shells, is said to be of marine origin. The fifth formation, from its containing principally fresh water shells, is conjectured to have been deposited from the water of a lake. The sixth, the alluvial formation, has been formed in the same manner as other alluvial deposites. According to Cuvier and Brongniart there appears to have been an alternate appearance and disappearance of fresh and salt water, an opinion which is not borne out by the facts stated in the essay. The opinions of Braard, La Metherie, and others, in regard to the kind of fluid from which these strata have been deposited, like the hypotheses of the authors of the essay, are sufficiently ingenious, but unsatisfactory.

Having premised this short account of the formations around Paris, we shall next notice some objections that have been started against the Wernerian geognosy, from the appearances presented by these rocks. It has been said, " The authors of the description of the country around Paris have themselves remarked that the appearances exhibited in that country are not consistent with the doctrines of the Wernerian school. We must add, that to us they appear most adverse to the theory of universal formations, the favourite and distinguishing dogma of that school. Eleven formations are here enumerated, and shown to succeed one another in one uniform order. They do so, however, only over a certain tract; and have none of them the least pretensions to be reckoned universal." The authors of the essay have in no part of it said that the appearances they describe are inconsistent with the doctrines of the Wernerian school: on the contrary, it is evident that they consider their descriptions as adding a new set of rocks to that system, by means of which they have been able to render their investigations so interesting. It is true that a considerable series of formations succeed one another in one uniform order; but they are not confined to a small tract of country; part of the series has already been traced through France to the confines of Switserland, and by one of the authors of the essay; and we are informed by Dr. Steffens, that the gypsum of Mont Martre occurs at Kiel in Holstein, on the shores of the Baltic: and now that the attention of mineralogists has been particularly directed to these rocks, we may expect to hear of their being found in other quarters of the globe. Even allowing, for the sake of argument, that this series of rocks had not been traced further than the gates of Versailles, I ask, should we be entitled from thence to conclude, from the mere extent of the mass, that it would not on examination prove to be an universal formation? I apprehend we could not, for this reason, that many of the formations now acknowledged to be universal were at first observed extending over very inconsiderable tracts of country. But the formations might be local ones, and therefore would not extend far; and yet such an appearance, instead of militating against this doctrine of the Wernerian school, would be an illustration of the truth of it.

It has further been remarked "that this same survey of the country around Paris is equally adverse to another doctrine of the school of Freyberg, closely connected with the former. The mineralogists of that school, it is said, have boldly ventured to assign to *every stratum* its individual place; and to fix, with more than prophetic skill, the order in which the different formations of the mineral kingdom will be found to succeed one another over the globe. If these pretensions are well founded, nothing in the science of mineralogy can be so valuable as the knowledge they must confer: if they are ill founded, nothing can be more pernicious than the errors into which they will betray. Every thing, therefore, is of importance, that brings them to the test of experience. Now it is remarked by Brongniart, that the order laid down by Werner is inverted in the case of the chalk. That substance is made the fifth of the floetz formation, and is placed above the highest floetz gypsum. Here, however, it appears far below it, with several formations between. The rule of Werner, therefore, does not hold in this instance; and it has been proved, that though the gypsum of Mont Martre agrees pretty nearly in its mineral characters with the newest gypsum formation of Werner, it differs entirely in its geological position. Again: the chalk described in this essay is not only covered by gypsum, but by limestone, and the gypsum by a second stratum of limestone and of sandstone, besides the siliceous millstone; all which is quite inconsistent with the Wernerian arrangement. All this shows how very imperfect that arrangement is, notwithstanding its pretended infallibility." If the limestone and gypsum of this series of rocks had been precisely the same with the second floetz limestone, and the second floetz gypsum, then there might have been a shadow of plausibility in the remarks just stated; but the preceding descriptions demonstrate that they differ most completely from these formations, not only in their oryctognostic, but also in their geognostic relations. Brongniart, indeed, was so convinced of the truth of this, that far from viewing them as proofs of the fallacy of the geognosy, he describes both the limestone and gypsum as new and distinct formations: the one he names coarse limestone, to distinguish it from the older floetz limestones: the other he names the third floetz gypsum, to show that he considers it as different from the fibrous, or second floetz gypsum; and he places both above chalk. (Vid. Brong. Mineralogie.) If Werner had had the folly and presumption to maintain that his system was complete, and that no other rock was to be discovered, that therefore he had fixed and ascertained the individual place of *every stratum around the whole globe*, he would have justly merited the severe and bitter censure of the reviewer of the essay of Cuvier and Brongniart.

But the author of the remarks is not satisfied with this commentary on the system itself; in his zeal he ventures still further, and maintains that the disciples of the Wernerian school so cloud their descriptions of the mineralogy of countries with a barbarous and uncouth nomenclature, that we must turn from them in disgust. He says, "The clearness with which this essay is written, and the absence of all technical language, except where it is absolutely necessary, we consider as great recommendations. The geologists of the Wernerian school follow a method directly opposite to this; they affect a phraseology peculiar to themselves, and employ a vocabulary, of which the harsh and uncouth terms, when closely examined, have not the precision to which every other consideration appears to be sacrificed. Descriptions drawn up in this way excite little interest, and render a branch of knowledge extremely inaccessible, which in its own nature is calculated to be very generally understood. The darkness which the language of Werner has thrown round all his doctrines seems as if intended to protect them from the eyes of the vulgar and uninitiated; and it may be doubted whether the Eleusinian rites threw a darker veil over the opinions of the Greek mystics, than the vocabulary of Freyberg does over the dogmas of the Saxon school. The consequence is, that of all the mineralogical descriptions which the Wernerian school has produced, we are persuaded none will be found so satisfactory as that which is now before us."

If this Wernerian nomenclature be so barbarous and unseemly, so totally unfit for the purposes of science, and so repulsive to good taste, how does it happen that Cuvier and Brongniart, so justly panegyrised by the reviewer, use it throughout their whole essay. The technical words that occur in it are but few in number, because the series of rocks consists of but few separate species, and they do not include many simple minerals. The following are the rocks and minerals mentioned in the treatise: chalk, limestone, marl, gypsum, clay, sand, sandstone, millstone, menelite, hornstone, flint, jasper, and silenite. Now this nomenclature is precisely the same as that used by the Wernerian school. Even the reviewer himself, in spite of his antipathy to every thing Wernerian, is forced to use the same nomenclature;

for he speaks of transition rocks, greenstone, &c. &c.; terms which he formerly considered as barbarous in the extreme, and worthy the school where they originated. But not only is the nomenclature for rocks and simple minerals used in this essay the same as that employed by Werner, but the authors also invariably employ his geognostic phraseology; thus the word formation is used throughout in the Wernerian signification, and the fundamental rock or bason of the district is described according to the method of the geognosy. Even the order followed in the description of the formations is that of the Wernerian school, beginning with the oldest, and finishing with the newest; and the difficulties that occur in the investigation are resolved by an appeal to the rules and method of the geognosy. The very map which is attached to the essay is executed according to the plan of Werner; and its title shows that Cuvier and Brongniart do not consider the nomenclature as barbarous, for it is entitled a geognostic, not a geological map.

If then this essay be so pure in its nomenclature, and perfect in its descriptions; and if it owes this to the language used, and the method of investigation pursued; it follows that the Wernerian nomenclature, and mode of investigation, although contrary to the intention of the author of the remarks, is proved to be the best, and that which must be employed if our mineralogical investigations shall attract any notice from philosophers, or regard from those interested in the mineralogical surveys of countries.

Lastly, the author of these remarks touches on a subject of high importance in geognostical inquiries; it is the study of the natural history of shells, as an accessory branch of geognosy. I cordially agree with him in opinion that conchology is a branch of natural history which cannot be sufficiently recommended to the attention of all geognosts, as furnishing important means of ascertaining with accuracy many of the leading facts in the history of the globe. It is a branch of natural history which has been long studied in Germany and France, and has of late years, particularly since its importance in geognosy has been ascertained and pointed out, made great advances. But we naturally inquire to whom are we indebted for our present highly interesting views of the natural history of fossil organic remains in general? It is to Werner. More than 30 years ago he first embodied all that was known of petrifactions into a regular system. He insisted on the necessity of every geognostical cabinet containing also an extensive collection not only of shells, but also of the various productions of the class zoophyta, of plants, particularly of sea and marsh plants, and ferns; and an examination of the remains of quadrupeds in the great limestone caves in Germany, soon pointed out to him the necessity of attaching to the geognostical cabinet also one of comparative osteology. As his views in geognosy enlarged, he saw more and more the value of a close and deep study of petrifactions. He first made the highly important observation, that different formations can be discriminated by the petrifactions they contain. It was during the course of his geognostical investigations that he ascertained the general distribution of organic remains in the crust of the earth. He found that petrifactions appear first in transition rocks. These are but few in number, and of animals of the zoophytic or testaceous kinds. In the older floetz rocks they are of more perfect animals; and in the newest floetz and alluvial rocks, of birds and quadrupeds, or animals of the most perfect kinds. He also found that the oldest vegetable petrifactions were of marine plants, the newer of large trees. A careful study of the genera and species of petrifactions disclosed to him another important fact, viz. that the petrifactions contained in the oldest rocks are very different from any of the genera or species of the present time: that the newer the formation the more do the remains approach in form to the organic beings of the present creation, and that in the very newest formations fossil remains of the present existing species occur. He also ascertained that the petrifactions in the oldest rocks were much more mineralised than the petrifactions in the newer rocks, and that in the newest rocks they were merely bleached or calcined. He found that some species of petrifactions were confined to particular beds, others were distributed throughout whole formations, and others seemed to occur in several different formations; the original species found in these formations appearing to have been so constituted as to live through a variety of changes which had destroyed thousands of other species, which we find confined to particular beds. He ascertained the existence of fresh water shells in solid strata, sometimes alone, sometimes intermixed with marine productions. These highly interesting observations having become generally known by means of his pupils, gave a stimulus to the study of petrifactions, which in a few years produced important results. They attracted the particular attention of the mineralogist, and roused the curiosity of the zoologist and botanist. They saw before them a wide field of the most interesting nature. The mineralogist confidently anticipated from this study important elucidations in regard to the various changes the earth has undergone, during the progress of its formation, from the earliest periods to the present time. The zoologist and botanist, by the discovery of new genera and species, hoped to increase the number of natural families, to fill up gaps in the present systems, and thus to perfect more and more the natural system of animals and plants. But this was not all. The philosophic naturalist soon saw that these investigations would also lead to much curious information in regard to the former physical and geographical distribution of plants and animals, to the changes which the animated world in general, and particular genera and species, have undergone, and probably are still undergoing; and he would naturally be led to speculate on the changes that must have taken place in the climate of the globe during the various changes and revolutions. The writings of Blumenbach, Von Hoff, Cuvier, Brongniart, Steffens, and other naturalists, are proofs of what has been done by following up the views of Werner.

In consequence of the pressure of matter this month we are obliged to defer, till our next number, the continuation of the Report on Vaccination.

Article VIII.

Analyses of Books.

(*Continued from* No. I. p. 63.)

Philosophical Transactions of the Royal Society of London for the year 1812.—Part II. This part contains the following papers:—

XII. *Observations of a Second Comet, with Remarks on its Construction.* By William Herschel, LL.D. F.R.S.

XIII. *Additional Experiments on the Muriatic and Oxymuriatic Acids.* By William Henry, M.D. F.R.S. V.P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester.] Dr. Henry, in the year 1800, had published an interesting paper in the Philosophical Transactions, on the change produced upon muriatic acid gas standing over mercury, by passing electrical shocks through it. The result was the evolution of hydrogen gas, and the conversion of part of the mercury into calomel. These changes he ascribed to the decomposition of water contained in the gas. The quantity of hydrogen gas evolved could not be made to exceed a certain proportion. This he ascribed to the complete decomposition of all the water contained in the gas. The effect would cease of course when the cause was removed. The object of the present paper is to correct some mistakes into which he had fallen, and to state the results of a repetition of the experiments with a more perfect apparatus. The following are the facts contained in the paper:

1. The quantity of hydrogen evolved from muriatic acid gas, by electricity, is the same whether the gas be exposed to the action of dry muriate of lime or not. 2. No heat is evolved by exposing dry muriate of lime to muriatic acid gas. 3. The

greatest quantity of hydrogen gas that can be produced when muriatic acid gas is electrified over mercury amounts to about $\frac{1}{15}$th of the bulk of the original gas. 4. When the gas is electrified in close vessels, both hydrogen and oxymuriatic acid gas are evolved, probably in equal bulks, and the greatest amount of both is $\frac{1}{35}$th of the bulk of the original gas. 5. The presence of hydrogen gas, mixed with the muriatic acid gas, prevents the evolution of any more hydrogen gas by the action of electricity. 6. When oxygen and muriatic acid gas are electrified together, water is formed, and oxymuriatic acid gas evolved. In what proportions it was found impossible to determine.

XIV. *Of the Attraction of such Solids as are terminated by Planes; and of Solids of greatest Attraction.* By Thomas Knight, Esq.

XV. *Of the Penetration of a Hemisphere by an indefinite number of equal and similar Cylinders.* By Thomas Knight, Esq.

XVI. *On the Motions of the Tendrils of Plants.* By Thomas Andrew Knight, Esq. F.R.S.] Mr. Knight's observations were made on the Virginia creeper (the *ampelopsis quinquefolia* of Michaux), the ivy, the common vine, and the pea. The tendrils uniformly receded from the light, and bent towards opake objects. Mr. Knight explains this by supposing that the side of the tendril acted on by light is elongated more than the other. This of necessity occasions a bending from the light. The contact of the tendril with any substance forces the nutritive juices of the tendril towards the side farther remote from the opake body. Hence a farther elongation of that side, and a consequent twisting of the tendril round the opake body.

XVII. *Observations on the Measurement of Three Degrees of the Meridian, conducted in England by Lieut.-Col.* William Mudge. By Don Joseph Rodriguez.] The determination of the figure of the earth has at all times attracted the curiosity of mankind; attempts to ascertain which were even made by the ancients: but our ignorance of their measures makes it impossible for us to draw any information from these attempts. Huygens and Newton were the first who reduced to the known laws of mechanics the principles on which the figure of the earth should be determined. The Academy of Sciences of Paris had the glory of first applying these principles to practice, by their celebrated measurements, performed in Peru, Lapland, and France. The conclusions of Newton and Huygens were verified by these measurements, and it was concluded that the figure of the earth was a spheroid flat at the poles, and jutting out towards the equator. Of late several new measurements have been made with more accurate instruments, greater skill, and more care if possible than the former ones. The French philosophers, at the commencement of the revolution, measured the whole length of

France, from Dunkirk to the Mediterranean. They even continued the measurement to Barcelona, and in 1806, Biot and Arrago continued it as far as Formentera, the farthest south of the Balearic islands. In the year 1801 the measurement formerly made in Lapland was repeated with great care and skill by three members of the Swedish Academy. And about the same time a similar measurement was made by Major Lambton on the Coromandel coast. All these measurements agree with each other in making the earth an oblate spheroid, and in showing that the length of a degree is continually increasing as we advance from the equator to the pole.

But about the same time another measurement was made in England of 2° 50′, which totally disagreed with all the others, and which represented the length of a degree as diminishing instead of increasing from the equator to the pole. This measurement was made by Lieut.-Col. Mudge, a gentleman perfectly well qualified for the undertaking, and with instruments of the most perfect construction that had ever been finished by any artist. Various opinions were formed about the cause of this difference. Some philosophers were disposed to give up the eliptic figure of the earth altogether, and to consider it as a body of an irregular shape. Others, and among these was Col. Mudge himself, conceived that it might be owing to local attractions, occasioning a deviation of the plumb-line. The object of this paper is to show that the want of coincidence in Col. Mudge's measurement with that of the other measurements, was owing to errors committed in determining the latitude of the places, particularly of Arbury-hill, which lay about half way between the two extremities of the arc measured. This proposition Don Rodriguez makes out in a satisfactory manner. His reasoning indeed is founded on the elliptic hypothesis; but he shows that no supposed alteration in the excentricity can produce any material variation in his conclusions. He deduces 57074 toises as the mean length of the degree in the latitude measured by Colonel Mudge. This subject deserves the closest attention of philosophers. We do not see why the astronomical observations of Col. Mudge might not be repeated. If an error in them could be detected it would go far to set this difficult and long agitated question at rest.

Don Rodriguez concludes his paper by drawing the attention of English philosophers to the measurement of arcs of the meridian in the southern hemisphere. At present we are possessed of only one measurement of this kind, that of Lacaille at the Cape of Good Hope, and it by no means agrees with similar measurements in the northern hemisphere. Hence it has been conceived by some that the northern and southern hemispheres have different excentricities. Lacaille's measurement should be repeated, and measurements should be made at Botany Bay and in Brazil.

XVIII. *An Account of some Experiments on different Combinations of Fluoric Acid.* By John Davy, Esq.] Fluoric acid was originally discovered by Scheele, and if we except the properties which he detected, and the observations on the gas by Dr. Priestley, little was known respecting it till the publication of the *Recherches Physico-chimiques* by Gay-Lussac and Thenard. Many new and extraordinary properties of this acid were described in that interesting work; and since that time the attention of chemists has been very much turned towards it. In its pure state it does not seem capable of assuming the gaseous form. But when combined with silica or boracic acid, it readily assumes the aerial state. This singular property of fluoric acid, of combining with a variety of other bodies, and constituting with them new acids, is peculiar to that acid. It has not yet been accounted for in a satisfactory manner. We may soon expect a set of experiments on the subject by Sir Humphrey Davy, which will set the subject in a new point of view, and farther develope certain changes which it seems expedient to introduce into the present theory of chemistry.

Mr. John Davy's paper contains several valuable facts, a summary of which we shall now lay before our readers. We shall at the same time correct some numerical errors in the paper, resulting from Mr. Davy having assumed 31 grains as the weight of 100 cubic inches of common air at the temperature of 60°, and when the barometer stands at 30 inches. I suspect that I have myself in some measure contributed to occasion this mistake, by stating 31 grains as the weight of 100 cubic inches of common air. I was induced to adopt that number on the authority of Mr. Kirwan; but the result of Sir George Shuckburgh Evelyn's experiments, which I consider as the most accurate which have ever been made on the subject (as corrected by Mr. Fletcher. Nicholson's Journal, 4. 35.) is that 100 cubic inches of common air at the temperature of 60°, and when the barometer stands at 30 inches, weighs very nearly 30·5 grains.

1. What was formerly called fluoric acid gas, Mr. Davy calls silicated fluoric acid gas. Its specific gravity is 2·990, that of air being 1·00; and 100 cubic inches of it weigh 108·992 grains. It is, therefore, the heaviest gas known except phosgene gas. This gas is composed of 100 fluoric acid, and 165·88 silica. Water absorbs 363 times its bulk of this gas. When absorbed by water, about a third part of the silica is thrown down, and subsilicated acid remains composed very nearly of equal weights of acid and silica.

2. Both of these acids combine with ammonia; 100 measures

of silicated fluoric acid absorb 200 measures of ammoniacal gas. Hence the resulting salt is composed of

Ammonia	24·83
Silicated fluoric acid	75·17
	100

Or, very nearly of three parts by weight of acid, and one part of ammonia. When dissolved in water, a portion of the silica is separated, and subsilicated ammonia remains composed of

Ammonia	26·53
Acid	73·47
	100

This last salt has a sharp taste, and crystalizes in four-sided prisms. It is very soluble in water, but does not deliquesce. When heated on glass or porcelain, it corrodes them, and seems to reproduce silicated fluate: it is decomposed by the alkalies and by sulphuric and muriatic acids.

3. Fluate of ammonia may be obtained by decomposing subsilicated fluate of ammonia, by means of ammonia. It is composed of

Ammonia	76·93
Acid	27·07
	100

This salt deliquesces. When heated, part of the ammonia is driven off, and a subfluate of ammonia remains. Its fumes are very dangerous when inhaled. When heated in a metallic vessel it sublimes unaltered, but it corrodes glass and porcelain, and combines with a portion of silica.

4. Fluoboracic acid may be formed by heating in a retort a mixture of one part by weight of fused boracic acid, two parts of fluor spar, and 12 parts of sulphuric acid. Its specific gravity is 2·370, that of air being 100; and 100 cubic inches of it weigh 72·31 grains. Water absorbs 700 times its bulk of this gas, and forms a heavy viscid acid, having considerable resemblance in appearance to sulphuric acid. Sulphuric acid absorbs 50 times its bulk of this gas, and forms a very fuming, tenacious acid liquor. Boracic acid decomposes silicated fluoric acid taking the place of the silica.

5. Fluoboracic acid combines in three proportions with ammonia. 100 measures of the acid condense 100 measures, or 200 measures, or 300 measures of ammoniacal gas. The first of these salts is solid, but the other two, though free from water, are liquid and colourless. These salts are composed as follows:

	Acid.	
The solid compound of	100	+ 24·89 ammonia.
The first liquid	100	+ 24·89 × 2 = 49·78
The second liquid	100	+ 24·89 × 3 = 74·67

If the analyses contained in this paper be near approximations to the truth, it would seem that the weight of a particle of fluoric acid is only 4·5, that of a particle of fluoboracic acid 25·7, which gives us 21·2 for the weight of a particle of boracic acid. The weight of an atom of silica would be 7·46, supposing silicated fluoric acid gas to be composed of one atom of acid, and one atom of silica. But a greater number of results would be necessary before any great dependance could be placed on their numbers.

XIX. *On a Periscopic Camera Obscura and Microscope.* By William Hyde Wollaston, M.D. Sec. R. S.] This paper contains an account of an ingenious improvement of the camera obscura by Dr. Wollaston, whose mechanical inventions of different kinds have been of so much service, not only to science but to mankind in general. The fault of the camera obscura is the want of distinctness in the images towards the margin of the field, owing to the figure of the glass, which is usually a double convex lense. Dr. Wollaston removes this defect, at least in a very considerable degree, by substituting a meniscus concave towards the objects, and convex towards the eye of the spectator. This is placed at a certain distance within the camera, and the light is allowed to enter by a round hole, which regulates in measure the extent of the field of vision, and the quantity of light introduced. His improvement of the common simple microscope, consists in placing a piece of metal with a hole in it between two plano-convex glasses ground to the same axis. This admits a much greater opening, without preventing distinct vision, and of course more than compensates the diminution of light arising from the construction of the instrument. By making the upper surface of the quadrangular prism, which serves to reflect the image of the object in the camera lucida, convex, that instrument becomes capable of giving a more correct image of a considerable field than was before the case.

XX. *Further Experiments and Observations on the Influence of the Brain in the Generation of Animal Heat.* By B. C. Brodie, Esq. F.R.S.] The result of Mr. Brodie's previous experiments, an account of which was given in the first number of this Journal, was that animal heat depends upon the functions of the brain, and that when these functions are destroyed, though respiration be kept up as usual, and though the air inspired undergoes the usual chemical changes, yet the temperature of the animal diminishes more rapidly than it would do if respiration were not kept up at all. Before this conclusion could be admitted, it was necessary to ascertain in a more satisfactory manner than he had done, how far the chemical changes which take place upon the air in artificial and in natural respiration were of the same extent. This is the object of the present paper. Seven experiments are related, three upon rabbits in their natural state, and four upon rabbits in which the functions of the brain were destroyed or suspended.

Most of our readers we presume know that the only sensible change produced upon air by respiration is the conversion of a portion of its oxygen into carbonic acid gas, without any change in its bulk. If, therefore, the same proportion of oxygen be changed into carbonic acid gas in both cases, then it follows that the air has undergone the same chemical changes. The following are the results of Mr. Brodie's experiments.

Natural Respiration.

1. A rabbit 50 cubic inches in bulk, converted in an hour 50·6 cubic inches of oxygen into carbonic acid gas.

2. A rabbit of 48 cubic inches, in the same space of time converted 56·44 cubic inches of oxygen into carbonic acid gas.

3. A rabbit of 48 inches, produced in the same space of time 56·44 cubic inches; or the same as the last.

Artificial Respiration.

1. A rabbit of 50 cubic inches converted in an hour 40·48 cubic inches into carbonic acid. But in this case some blood was lost, and the extent of the circulation was somewhat diminished.

2. A rabbit of 45 cubic inches produced in an hour 51·1 cubic inches of carbonic acid gas.

3. A rabbit of 48 cubic inches produced in an hour 54·43 cubic inches of carbonic acid gas.

4. A rabbit of 47 cubic inches produced in an hour 56·55 cubic inches of carbonic acid gas.

Such are the results of Mr. Brodie's experiments. They seem to make out his conclusion in a very satisfactory manner, that the same chemical changes were produced upon the blood by artificial respiration that take place during natural respiration. Now, in all these cases, the thermometer sunk more rapidly in the animal subjected to artificial respiration than in an animal killed at the commencement of the experiment and placed in the same circumstances.

Are we then to give up the opinion that respiration produces animal heat, and to conclude that the chemical theories of animal heat at present received are all erroneous? The question is of very difficult answer. There need be no very great hesitation in admitting that Dr. Crawford's experiments, which constitute the foundation of his theory, are of too delicate a nature to admit of precision. But it is not the experiments on arterial blood, though these are the only ones alluded to by Mr. Brodie; it is the experiments on the specific heat of the aerial bodies connected with respiration that constitute by far the most difficult part of the investigation. If Dr. Crawford's statements respecting the specific heats of oxygen gas, atmospherical air, and carbonic acid gas, be admitted, we do not see how it is possible to reject the conclusions to which he has come. Let us take his statements for granted, and see what will follow. According to him the specific heats of these gases are as follows:

Oxygen gas	4·749
Air	1·790
Carbonic acid gas	1·0454

Now let us take any one of Mr. Brodie's experiments, as, for example, the second. The rabbit converted $\frac{1}{18}$th of the bulk of the air into carbonic acid gas; or, which is the same thing, $\frac{1}{18}$th of the oxygen was changed into carbonic acid. Of course, its specific heat was sunk from 4·749 to 1·0454. To get a base of calculation, let us suppose the absolute quantity of heat in bodies at the temperature of 65° to be 1500 degrees. In that case, no less than 1170 degrees of heat will be disengaged merely by the change of the oxygen into carbonic acid gas. This quantity has to be divided over 18 parts of an air consisting of a mixture of

79	azotic gas
15·5	oxygen gas
5·5	carbonic acid gas
100·0	

Now the specific heat of this mixture will be obtained by multiplying these numbers respectively into the specific heat of each of these gases, adding the products together, and dividing the sum by 100. This operation gives us the specific heat of the residual mixture, after respiration, 1·420. The 1170 degrees of heat divided into 18 parts gives 65 degrees for each part; but as the specific heat of the residual gas is 1·42, these 65 are only capable of producing an increase of temperature equivalent to 45·7 degrees. The air, when drawn in, was 65°; it ought, therefore, when thrown out, to be of the temperature of 110°·7; but as its temperature is only about 100°, there are 10·7 degrees of heat to be accounted for, and which can only be accounted for by supposing it to be absorbed by the body. If we were to suppose, with Mr. Dalton, that the absolute heat of bodies amounts to 6000 degrees, in that case no less than 4679 degrees would be disengaged from the oxygen by its change into carbonic acid. This ought to heat the residual air up to the temperature of 247·7 degrees; so that no less than 147·7 degrees must have entered the body.

I do not, for my own part, lay any stress upon these estimates, because I consider the whole doctrine relative to the absolute heat of bodies as destitute of foundation, and because I conceive Dr. Crawford's experiments to determine the specific heat of the gases as too delicate to admit of precision. They are stated merely to show that Dr. Crawford's theory does not depend alone upon his estimate of the specific heat of arterial and venous blood. It is the result of a very numerous train of experiments, investigated with much labour and ingenuity, and all admirably coinciding in one point. The difference between the specific heat of arterial and venous blood, serves only to account for the circumstance that the temperature in the lungs is not higher than in other parts of the body, and to explain in what manner the heat absorbed in the lungs is gradually developed during the circulation.

Independent of Dr. Crawford's experiments, there are other circumstances which throw considerable plausibility upon the opinion that heat is evolved by respiration. Every body knows that those animals that do not perspire are cold blooded, and that in ourselves the temperature increases when we respire more rapidly than usual, and likewise when the circulation becomes accelerated. The change of oxygen gas into carbonic acid gas is accompanied with an evolution of heat in all other cases with which we are acquainted. This holds in combustion in a remarkable degree. It holds no less remarkably during the fermentation of malt liquors. Why should it not hold equally in respiration?

These circumstances are not sufficient to set aside Mr. Brodie's experiments, which seem to have been made with great care, and which are very satisfactory; but they ought to induce us to consider the subject on all sides before we embrace the conclusion to which these experiments seem to lead. Living bodies do not seem to be subject to the same laws as dead matter. Mr. Brodie has shown that animal heat depends upon the action of the brain. Dr. Currie, in a very interesting paper published in the Philosophical Transactions more than 20 years ago, but to which physiologists have hitherto paid no attention, has shown, by experiments little less conclusive than those of Mr. Brodie, that the evolution of heat is connected with the action of the stomach. I shall take the first opportunity that occurs to reprint Dr. Currie's paper in a future number of the *Annals of Philosophy*, in order to contribute as far as in my power to draw the attention of physiologists to a very curious subject still imperfectly understood.

XXI. *On the different Structures and Situations of the*

Solvent Glands in the Digestive Organs of Birds, according to the Nature of their Food, and particular Modes of Life. By Everard Home, Esq. F.R.S.] This is a very interesting paper, and throws considerable light upon a branch of comparative anatomy not hitherto much attended to. Sir Everard Home divides the digestive organs of birds into four parts. The first is the dilatation of the œsophagus, which forms a reservoir for the food, and which is called the *crop*. The second is the part into which the ducts of the solvent glands open, and which he calls the *cardial cavity*. The third is the cavity embraced by the digastric muscle, or gizzard. The fourth is the space between the opening of the gizzard and beginning of the duodenum, which he calls the *pyloric cavity*; though in some instances, he says, it hardly deserves that name. He describes the structure of these organs in the golden eagle (*falco chrysaetos*), the sea eagle (*falco ossefragus*), the hawk (*falco nisus*), the soland goose (*pelecanus bassanus*), the heron (*ardea cinerea*), the cormorant (*pelecanus carbo*), the sea gull (*larus canus*), the woodpecker (*picus minor*), the little ant (*alca alle*), the pigeon (*columba domestica*), the swan (*anas cygnus*), the goose (*anas anser*), the common fowl (*phasianus gallus*), the turkey (*meleagris gallipavo*), the parrot (*psittacus æstivus*), the casowary (*casuarius emeu*), the American ostrich (*rhea Americana*), the African ostrich (*struthio camelus*).

XXII. *On some Combinations of Phosphorus and Sulphur, and on some other Subjects of Chemical Inquiry.* By Sir Humphrey Davy, Kt. LL.D. Sec. R.S.] This paper contains the following valuable additions to our knowledge of the combinations of phosphorus and sulphur.

1. Phosphorus combines with two proportions of chlorine. The first of these is a limpid liquid; the second a white sublimate. To the first of these Sir H. Davy has given the name of *phosphorane*. It may be formed by passing the vapour of phosphorus through corrosive sublimate. It is composed of 100 phosphorus united to $333\frac{1}{3}$ of chlorine. It dissolves phosphorus. The sublimate, called *phosphorana*, is composed of 100 phosphorus united to $333\frac{1}{3} \times 2$ of chlorine, or $666\frac{2}{3}$.

When phosphorane is mixed with water, aud slowly evaporated, crystals in the form of four-sided prisms make their appearance. These consist of phosphorous acid combined with water. Phosphorana, treated with water in the same way, forms a thick viscid substance, which consists of phosphoric acid united with water.

2. When these crystals of *hydrophosphorous acid*, as Sir Humphrey Davy calls them, are heated, they are converted into phosphoric acid, and a peculiar gas escapes, to which he has given the name of *hydrophosphoric gas*.

Hydrophosphoric gas is not spontaneously combustible; but it explodes when mixed with air, and heated to a temperature rather below 212°. Its specific gravity is 0·87, that of air beirg 1·00: 100 cubic inches of it, under the ordinary pressure and temperature, weigh 26·53 grains. Its smell is disagreeable; but not so much so as that of phosphureted hydrogen gas: three measures of it require rather more than five measures of oxygen gas for complete combustion. When potassium is heated in it, its bulk is doubled, phosphuret of potassium is formed, and the residual gas is hydrogen. When sulphur is heated in it, the bulk is also doubled, sulphureted hydrogen gas formed, and a compound of sulphur and phosphorus remains. Hence the gas is a compound of 4·5 hydrogen and 22·03 phosphorus, or of 100 hydrogen and 489·56 phosphorus.

3. When phosphorus is converted into phosphoric acid, by combustion in oxygen gas, every grain of phosphorus consumes $4\frac{1}{2}$ cubic inches of oxygen. Hence phosphoric acid is composed of 100 phosphorus united to 150·5 oxygen. Phosphorous acid contains just half the oxygen present in phosphoric acid, or it is a compound of 100 phosphorus and 75·25 oxygen.

4. When phosphorus is slowly burnt in the air, the liquid produced is a mixture of phosphoric and phosphorous acids. When phosphorus is burnt in rare air at a moderate heat the solid acid produces phosphorous acid.

5. The specific gravity of sulphurous acid gas is 2·193, that of air being 1·000; and 100 cubic inches of it under the usual temperature and pressure weigh 66·89 grains. It is composed of equal weights of oxygen and sulphur. When oxygen gas is converted into sulphurous acid gas the bulk is not altered.

6. The specific gravity of sulphureted hydrogen gas is 1·177, that of air being 1·000: 100 cubic inches of it, under the common temperature and pressure, weigh 35·89 grains. It is composed of 100 parts, by weight, of hydrogen and 1509 of sulphur.

7. Sulphuric acid, free from water, does not appear possible to be formed. Dry sulphurous acid gas and nitrous acid gases have no action on each other.

8. The liquid compound of sulphur and chlorine, which I discovered about eight years ago, is composed of 30 sulphur and 67 chlorine.

9. Water has the property of combining in definite proportions with a great number of bodies, and it has a considerable effect on their properties. In this manner it combines with the earths, alkalies, and most of the metallic oxides.

Article IX.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Vegetables found in some species of Minerals.*

Professor Blumenbach, of Gottingen, in a letter to Von Moll, says, that though he had hitherto disbelieved the occurrence of vegetable bodies in the dendritic variety of chalcedony named *mocha stone*, he must now admit that it does sometimes contain true vegetables, apparently of the nature of *conferva*. He observed these in specimens from Iceland and Catherinenburg. The same celebrated zoologist received from Dr. Lichtenstein, the traveller, a very remarkable agate, which was worn as a precious amulet by a Japanese prince. On examining it, he discovered in its interior the fructification of an unknown plant, somewhat resembling the *sparganium erectum*.

II. *Turquois.*

Dr. Langsdorf, who accompanied Captain Krusenstern in his voyage round the world, presented to Blumenbach an uncut turquois, from Nischabar, in Eastern Persia. From the specimen it appears certain that this substance is not a petrifaction, but a particular mineral species which occurs in nests in beds of clay. According to the analysis of Dr. John it appears to be nearly allied to mollite (siderite).

III. *Chromium in Chlorite.*

Mr. Bergweser Uttinger, of Sontkofen, has discovered from 1 to $1\frac{1}{2}$ per cent. of chromium in a green coloured mineral heretofore considered as chlorite, and which gives a green colour to some kinds of sandstone. This mineral occurs in masses, from the size of an inch to that of a pea, in grey shell limestone and grey sandy marl. It occurs also disseminated in compact and lenticular clay iron stone. It thus appears that chromium is far from being circumscribed in its distribution.

IV. *Italian Rocks.*

Mr. Giuseppe Gautieri, principal inspector of forests in the kingdom of Italy, has lately examined the interesting valleys of Fiemme, Fassa, and Livinalunga, and finds the rocks to belong to the floetz trap formation, and to resemble in every respect the trap hills near Verona, Vicenza, and Padua, which have been described by many mineralogists as volcanic.

V. *Giesèke, the mineral Dealer.*

This Gentleman was mentioned in a paper on Greenland minerals, by Mr. Allan, in our last number. We think a few more facts respecting him will be interesting to our readers. He has long been well known on the Continent as a dealer in minerals, adding to unbounded zeal a great knowledge of mineralogy. In the year 1806 he left Copenhagen, in a Danish ship, for Greenland, with the intention of collecting the valuable and rare minerals which occur in that remote region. While he was absent his valuable mineralogical collection in Copenhagen fell a sacrifice to the flames during the bombardment of that capital by the English. To add to his misfortunes, a considerable collection of the rarest minerals in Greenland, which he had shipped on board a Danish vessel for Copenhagen, was taken by a British cruizer. This is the collection described by Mr. Allan, in the second number of this Journal, and containing, as our readers will perceive, on the slightest inspection, several of the most curious minerals hitherto discovered. When Gieseke was informed of these disasters he resolved, although struggling with want, to prolong his stay in that country, in order to replace the rich collection which he had lost.

VI. *Sarcocol.*

It has been generally believed that the substance called sarcocol is an exudation from the *penæa sarcocolla*; but there is very good reason to doubt the truth of the opinion. It was first taken up by Linnæus, and has been maintained ever since upon the authority of his name. Professor Thunberg has published an account of the genus penæa in the *Gesellschaft Naturforschender freunde zu Berlin Magazin* for 1807, p. 121. The species are ten. They all grow at the Cape of Good Hope, and have been seen no where else. The penæa sarcocolla grows on the mountains of Hottentot Holland, and in those below the west side of the Table Mountain. Now Thunberg, who was on the spot, expressly affirms that sarcocol is neither collected nor known in that country.

It would have some tendency to throw light upon the plants that yield several resins, and gum resins, with the history of which we are at present unacquainted, if any wholesale druggist, or merchant, in London, who is in the habit of importing these articles, would state the countries from which they are brought.

VII. *Marine Transit.*

In reading over the description of the marine transit published in a late number of the Philosophical Magazine (Vol. xl. p. 401), there was a circumstance which struck me as likely to be of considerable importance. I do not know whether Mr. Chevasse has adverted to it, but at all events it can do no harm to draw his attention to it. When mercury is agitated in contact with the air it is gradually changed into a black powder (the protoxide). This must happen, to a certain amount, in the marine transit, from the continual running of the mercury, unless the air be excluded, which seems impracticable. Two methods of remedying this defect (which would interfere with the rate at which the mercury flows) present themselves. The first method would be to fill the whole apparatus with some gas destitute of oxygen, and as nearly as possible of the specific gravity of air. Azote is the gas that would answer best; but I doubt whether it would be practicable to confine azote in any such apparatus for a great length of time, to the complete exclusion of air, considering the great changes in expansion to which it would be liable. The other method is to have an apparatus for filtering the mercury through leather. By repeating this filtration once a week or so, and taking care to keep the apparatus and mercury always as dry as possible, I have little doubt that it might be preserved nearly in a constant state of purity. How far time may be accurately measured by the flowing of mercury, remains to be decided by experiment. It is well known that water is far from flowing equally; and accordingly the clepsydras, or water clocks, of the ancients, were of little value. Mercury certainly promises fairer; but I much doubt whether it be possible to regulate its rate of flowing so as to make it absolutely equable.

VIII. *Variation of the Compass.*

Though the fact, that the compass is constantly changing the direction towards which it points, has been known since the year 1645, when it was discovered by Gellibrand, or Mair, in England, yet we still know nothing about the laws according to which it varies. To give our readers some idea of this variation, we shall set down a few examples of the variation in London, during different years: 0, or no variation, means that the needle points due north. The figures indicate the number of degrees that it deviates from north:—

1580	11°	15′	0″	East	Burrows.
1622	6			....	Gunter.
1634	4	3	30	....	Gellibrand.
1657	0			....	Bond.
1672	2	30		West . ..	Halley.
1683	4	30		....	Halley.
1692	6			....	Halley.
1722	14	20		....	Graham.
1747	17	40		....	Graham.
1811	24	14	2	....	Lee.

There is reason to believe that the variation westward has now nearly reached its limit, and that it will very soon begin to move eastwards again. It would have a tendency to throw considerable light upon the subject, if it were ascertained that this change begins to take place in all parts of the earth's surface at the same time. Now, therefore, is the period to draw the attention of philosophers to this curious point. Those situated at some considerable distance from London will be enabled to make the most decisive observations. North America is well situated for the purpose; and so is Sweden and Russia. We hope that this important opportunity, which is not likely to recur again these 300 years, will not be allowed to escape without at least adding one fact to the little that we do know about magnetism.

IX. *Ulmin.*

I am informed by Mr. Smithson that some parts of my account of his paper on ulmin (see p. 73) are not accurate. He did not find the solution of ulmin to redden vegetable blues; but he found the solution of the substance separated from ulmin by acids to do so, though it retained none of the acid by which it had been obtained. It is not the ulmin itself which Mr. Smithson considers as related to extractive, but this same substance which acids separate from it, and whose solubility in water, as well as alcohol, &c. prevent it from being considered as a resin.

X. *Register of the Rain that fell (in Inches) in Scotland, for* 1812.

	Edinburgh.	Bothwell Castle.	Glasgow.	Greenock.	Largs.	Gordon Castle.
Jan.	1·475	1·462	1·352	1·828	2·412	1·32
Feb.	3·592	2·460	1·424	3·416	4·280	1·70
March	3·103	2·320	1·865	2·904	2·412	2·53
April	1·095	·974	·842	1·294	1·366	1·64
May	2·100	1·475	1·443	2·140	2·230	3·62
June	2·235	2·630	1·802	1·448	2·480	2·33
July	1·340	1·759	1·531	2·656	2·173	3·08
Aug.	3·403	2·510	2·166	3·733	3·394	1·48
Sept.	1·082	1·712	2·342	2·254	2·778	1·35
Oct.	2·822	3·549	5·345	3·944	5·381	6·83
Nov.	3·975	3·990	2·452	4·539	5·257	3·72
Dec.	·890	·156	·246	·720	1·049	1·17
Total	27·112	24·997	22·810	30·876	35·212	30·77
In 1811	32·64	33·099	27·801	......	56·597	31·34
1810	....	25·010	21·433	......	38·714	25·89

X. *Freezing of Alcohol.*

Mr. Hutton, of Edinburgh, by a new method, which he has not thought proper to make known, but which he conceives capable of producing an unlimited degree of cold, has succeeded in freezing alcohol of the specific gravity of 0·798, and even of a specific about as low as 0·790 at the temperature of 60°. The alcohol he conceives froze at the temperature of − 110°; but no stress can be laid upon any such determination, as the contraction of the alcohol at the instant of freezing is probably irregular. It divided into three layers. The uppermost, yellow, consisted probably of the oils to which spirit owe their flavour, and which could not be removed by the concentration. The lowermost was the alcohol, probably still retaining water. This is nearly a tasteless liquid, according to Mr. Hutton: what the middle stratum is, he does not mention. This discovery of Mr. Hutton is of moment, as it removes the only anomaly that existed against the general law, that all liquids becomesolid when exposed to a sufficient degree of cold.

Article X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 28th January a paper by Dr. Brewster was read, *On some New Properties of Light.* This paper contained a kind of abstract of the author's numerous and interesting experiments on light. It was divided into four parts. 1. On a new property of light discovered in the agate. In a plate of transparent agate about $\frac{1}{15}$th of an inch thick, cut in a direction perpendicular to the laminæ, besides the colourless image of the object seen through this substance, there was another image strongly coloured. On viewing this coloured image through a prism of iceland crystal, it was found to possess the properties of doubly refracted light, disappearing at the quadrants when the prism was turned round. The colourless image possesses the same property. When a ray of light passes obliquely through a thin plate of agate, if it be received upon another plate of agate cut parallel to its coats, it easily passes through; but if the plate be cut in a direction perpendicular to the coats, it is totally reflected. Carnelian and chalcedony, which are kindred substances, possess analogous properties. Besides the two images above-mentioned, there is a nebulous appearance always visible in agate, probably occasioned by its want of complete transparency. This nebulous appearance Dr. Brewster is at present examining, hoping that it will throw additional light upon this curious but intricate subject. 2. *On the Double Refraction of Chromate of Lead.* Dr. Brewster found that the double refrangibility of chromate of lead is nearly three times as great as that of Iceland crystal. 3. *Power of different Bodies to refract Light.* Since Newton's time it has been conceived that the diamond possesses this power in the highest degree; but Dr. Brewster has found that both chromate of lead, and realgar, exceed it considerably in this respect. 4. *Double Dispersive Power of Bodies.* Dr. Brewster has found that bodies possess two different dispersions, one considerably greater than the other. This he discovered at first, by comparing his own measurement of the dispersive power of a body with that of Dr. Wollaston.

On Thursday, the 4th February, a paper by Sir Charles Blagden was read, as an appendix to Mr. Ware's observations on short-sightedness. Sir Charles's own experience confirmed Mr. Ware's conclusions. When a boy of four years he was not short-sighted; but he was so, in a slight degree, at nine. He did not use glasses till 30. At first number 2 answered; then he required number 3; and at present the glass which he uses is number 5. He thinks that his short-sightedness has been increased by the use of glasses, and considers it as probable that if he had not employed glasses it would have gone off altogether.

At the same meeting was read an account of the late eruption of the volcano called the Suffriere, in the island of St. Vincent, by Mr. Hamilton, of Nevis, in the West Indies. This mountain is the highest, and farthest north, of a chain of mountains which runs through the island. Its height is 3000 feet. It had been famous for its eruptions in former times; but none have occurred since 1718. The crater on the top was a mile in diameter, of the form of an inverted cone, and about 800 feet deep. In the middle of it was a conical hill, covered with wood, except the top. The mountain was covered with wood. It got its name from the general quantity of sulphur upon it. Through some of these sulphureous deposites a white smoke was always seen to issue. Some smoke also issued from the little conical hill at the bottom of the crater. The great number of earthquakes, no fewer than 200, during the course of 1811, announced that the entrails of the mountain were in a state of great commotion; but it continued to be visited by the curious; and even so late as the 27th of April, a party of gentlemen went to it, and spent several hours in examining the crater. On the 28th the eruption began by a prodigious shower of ashes, which continued to be thrown up all day, and which darkened the air, and loaded the wings of the birds to such a degree as to bring them to the ground. On the 30th, flames were first seen to issue out, accompanied by a vast column of smoke, and frequent thunder and lightning. On the 1st of May a prodigious shower of stones fell all over the island for about an hour, killed and wounded many negroes, and did prodigious damage. Two eruptions of lava took place: one ran down the north side of the hill, and reached the sea, distant about four miles, in four hours: the other ran east, choked up a considerable river, and did infinite damage. Much of the wood was destroyed, and several estates were so much covered with stones and ashes as to render it almost impossible to clear them. The ashes fell upon a ship at sea, more than 200 miles distant from the island.—

On Thursday, the 11th February, Mr. Hamilton's account of the Suffriere, in St. Vincent, was concluded.

—Some time after, a prodigious eruption of mud took place. It was so immense as to fill up all hollows and precipices, and has made the whole country so smooth that a coach and six might now drive up the hill. It was attended with a violent earthquake. Mr. Hamilton accounts for these phenomena, by supposing that the rivers whose courses were blocked up made their way into the burning mountain, and occasioned both the eruption and the earthquake. The inhabitants of Nevis were informed of the eruption of the Suffriere by loud and regular explosions, so like an engagement at sea that several of the vessels were deceived by it; and the Commodore of a convoy, conceiving that privateers were cutting off his sternmost ships, gave chase to the supposed enemy. Vessels 100 miles east from Barbadoes had their decks covered with sand, while vessels much nearer St. Vincent escaped without any such appearance. This Mr. Hamilton thought at first could only be accounted for by some eruption out of the sea to windward of the ships, or in some part of South America; but he seems afterwards to have altered his opinion.

On Thursday, the 18th February, a paper was read by Sir Everard Home, Bart. *on the Narwal*, proving that the tusk is peculiar to the male. It has been believed by naturalists that the narwal has two tusks, and that one of them is frequently broken off. A scull with two tusks was exhibited in the Leverian Museum. When that museum was exposed to sale, this preparation was examined, and it was found that one of the tusks had been artificially fixed on. The tusk is upon the left side of the skull. On the right side there is the mark of a milk tusk which had been shed. In a conversation with Mr. Scoresby, jun. of Whitby, that gentleman told Sir Everard that sculls of the narwal were often found without tusks, and that it was a general opinion among the whale-fishers that the tusk was peculiar to the male. Sir Everard earnestly requested Mr. Scoresby to procure him such a skull, which he promised to do. He has lately sent him a skull without any tusk, and assured him that it was taken from a female narwal; so that the fact of the tusk being peculiar to the male is made out beyond doubt.

A paper by Dr. Wollaston was also read, showing *A Method of Drawing very fine Wire.* Muschenbroek mentions that an artist of Nuremberg drew gold wire so fine that 500 inches of it only weighed one grain; but he says nothing about the method. It may be done thus: take a wire of fine silver, of a moderate diameter, and drill a hole through its centre one-tenth of the diameter of the whole. Fill this hole exactly with a wire of fine gold. Draw out the silver wire, say to the fineness of $\frac{1}{300}$th of an inch. It is obvious that the gold wire will now be only $\frac{1}{3000}$ of an inch. Put the wire into warm nitric acid. The silver will be dissolved, and the gold wire will remain. It being difficult to drill a hole in fine silver, on account of its toughness, Dr. Wollaston tried platinum, and found it to answer. He made a mold for a silver wire, fixed in its centre a fine platinum wire, and then filled the mold with melted silver. The wire was now drawn out, and the silver dissolved by aqua fortis. By this means he easily obtained wires of the diameters of $\frac{1}{4000}$ and $\frac{1}{5000}$th of an inch, which is fine enough for every useful purpose. To make use of such wires the best way is to take the requisite length before the silver is dissolved off, to give it the shape of an U, make a hook at each extremity, suspend it by a gold wire, dip it into the aqua fortis, and leave a portion of the silver at the ends, which will make it much more manageable. There seems to be no limit to this method, except the ductility of the metals. Dr. Wollaston obtained platinum wire as fine as $\frac{1}{30000}$th of an inch; but it was impossible to obtain it thus fine of any length.

LINNÆAN SOCIETY.

On the 2d of February a description of a plant found in India by Col. Hardwicke was read, which he conceived to constitute a new genus. It was found in the Mysore, and transported by the Colonel to his garden, where it grew luxuriantly. It belongs to the natural order of *Malvaceæ*. Its greatest length was about 12 feet, and its stem was about 3 inches diameter. Hence it was too weak to resist violent winds when removed from the forest. It belongs to the class and order *Monodelphia Polyandria*. Its essential characters are, a double calyx, the outer of three leaves, the inner of five. The corolla has five petals. The seed-vessel is divided into ten cells, containing each about ten seeds in two rows. Col. Hardwicke, if it should turn out to be a new genus, proposes to name it after Lord Valentia, *Annesleyana*.

A description of seven new species of shells by Mr. Brooks, described from his own collection, was also read. The first a chama, which was figured by Gualtieri, but not hitherto considered as a new species. He calls it chama Gualtieri. Six species of halyotis. Mr. Brooks was not acquainted with the place where any of these were found, except one, and that was the Pacific Ocean. It is very much to be lamented that the habitation of shells, and the animals that inhabit them, are not more attended to by collectors. It is the only way to perfect this difficult department of natural history.

At the meeting which took place on the 16th February, the following communications were read :—

A letter from Mrs. Taylor, with specimens of a gelatinous plant, conceived to be a new species of lichen, which grew two successive years near her house in Devonshire.

A letter from Mr. Hoy, at Gordon Castle, Scotland, giving an account of a fish cast ashore near Gordon Castle, the trichiurus lepturus, not hitherto conceived to exist upon the British coast. The specimen, without the head, was about $12\frac{1}{2}$ feet long.

A letter from Dr. Smith, President of the Linnæan Society, giving an account of the astragalus campestris, a new British plant, lately discovered on a rock in Angus-shire, by Mr. George Don. It is a splendid plant, and quite alpine. It was described by Haller. In the Flora Danica it is figured under the name of astragalus uralensis. Wildenow describes it under the name of astragalus sordidus.

An account by Mr. Kirby of a new species of insect, from an imperfect specimen sent him by Mr. Sowerby.

An account of a tree observed in the West Indies by Mr. Push belonging to Jussieu's order of *Euphorbia*, and to the *Monoecia Monodelphia* of Linnæus. It constitutes a new genus. Mr. Push names it, in honour of Dr. Hosack, of New York, Hosackia laurina.

IMPERIAL INSTITUTE.

Account of the Labours of the French Imperial Institute for 1812.

(*Continued from p.* 156.)

Researches of M. Biot *on Light.*

In the notice published last year of the labours of the Class, we have given an account of the researches read to the Institute by M. Arrago on the colours exhibited by plates of mica, sulphate of lime, and rock crystal, when they are exposed to a *polarized* ray. Since that time M. Biot has presented to the Institute a suite of memoirs, in which he announces that he has discovered by experiment the exact laws of these phenomena, that he has expressed them by mathematical formulas, and that he has reduced them all to one general fact, from which all the phenomena may be deduced by calculation. We shall give an account of these researches of Biot in their order, beginning with the first memoir, which was read to the Institute on the 1st of June, 1812; and every page of which was marked by M. Delambre, perpetual secretary.

M. Biot, in the first place, contrived an apparatus with which all the phenomena may be exactly observed, and the angles measured. He makes the white light from the clouds fall upon a well-polished black glass, under such an angle that it is completely polarized by reflection. He makes the reflected ray pass through the tube of the telescope of a repeating circle, deprived of its glasses. The edge of the circle is placed vertically, and parallel to the plane of reflection. The upper extremity of this tube is enveloped in a circular drum, which turns stiffly round it. Its circumference is divided into 16 parts, each of which, of course, corresponds to an angle of 22° 30′. At the two opposite extremities of the same diameter are two branches of copper parallel to the axis of the tube, between which there is a circular plate of copper, which turns freely round an axis perpendicular to the two branches. This plate itself carries a ring, which turns freely round its surface, and round the axis of the tube. All these different motions may be regulated and stopped by screws. The crystalline plate, on which the observation is to be made, is placed upon this ring. It is evident, 1. That by turning the ring round its centre, upon the plate which carries it, the axis of the crystalline plate may be directed so as to form any angle whatever with the plane of polarization. 2. That by turning the plate of copper, which supports the ring the crystalline plate may be inclined at pleasure, with respect to the polarized ray. 3. That by turning the drum round the tube which it envelopes, the plane of incidence of the ray upon the crystalline plate may be placed at all possible azimuths round the plane of polarization. The crystalline plate may then be presented to the ray in all possible positions. The angles which determine these positions are measured by the divisions on the apparatus. The incidence of the ray upon the surface of the plate is likewise measured by bringing the plane of incidence into the plane of the edge of the circle, and raising the tube till the surface of the plane becomes exactly horizontal, which is ascertained by a very sensible level.

The first experiments of M. Biot were made with plates of sulphate of lime. The easy divisibility of this substance, and the possibility of reducing it into thin, equal plates, with surfaces perfectly parallel and finely polished, makes it exceedingly proper for exact observations. M. Biot began by endeavouring to discover the direction of the axis of double refraction. The primitive form of this substance assigned by Haüy is a rectangular four-sided prism, whose bases situated in the plane of the plates are parallelograms with angles of 113° 7′ 48″ and 66° 52′ 12″.

The theory of crystallization does not determine the lengths of the sides opposite to these angles. M. Haüy has determined them so as to represent the secondary forms as simply as possible, and has conceived them to be to each other in the ratio of 12 to 13. The axis of double refraction has no symmetrical relation to such a parallelogram; but if we triple the side 12, allowing the other to remain constant, the axis of double refraction will then pass along the greater diagonal of this new parallelogram, and will form an angle of 16° 13′ with the side 36.

This direction being known, M. Biot exposed thin plates of sulphate of lime, under an incidence perpendicular to a polarized ray; and analysed the light transmitted, making use successively and indiscriminately of a rhomboid of Iceland crystal, or of the reflection of a mirror. He observed two coloured images, as M. Arago had announced, and he ascertained that they possessed the following characters: 1. A part of the incident light, which we shall call E, is polarized by the plate; the remainder, O, preserves its primitive polarization. 2. The colour of the portion polarized by the plate is the same in what azimuth soever its axis is placed, relative to the plane of polarization of the ray. 3. When the light is analysed by means of a rhomboid of Iceland crystal, the principal sections of which is directed according to that plane, the ordinary image given by the rhomboid is constantly a mixture of the two colours O and E. The extraordinary image is always of the colour E; and the separation of the two colours is complete when the axis of the plate forms an angle of 45° with the plane of polarization of the ray.

M. Biot attempted at first to represent these phenomena by the same formulas that Malus had given for the intensity of the pencils given by the rhomboids of carbonate of lime. He found that this law would not apply. He endeavoured to discover the modifications which it must undergo, and by multiplying his observations every way he found the two formulas following, which represent all the phenomena. Let us suppose that the axis of the plate makes an angle i with the plane of polarization of the incident ray; let us suppose, likewise, that the transmitted light is analyzed by means of a rhomboid of calcareous spar, the principal section of which makes an angle α with the same plane. Let us call E the intensity of the portion of incident light which the plate polarizes, and let O be the complementary portion which preserves its primitive polarization. If we denote by F_o, F_e, the intensities of the two ordinary and extraordinary pencils, observed across the rhomboid, we shall have

$$F_o = O \cos.^2 \alpha + E \cos.^2 (2i - \alpha).$$

$$F_e = O \sin.^2 \alpha + E \sin.^2 (2i - \alpha).$$

If we wish to analyse the transmitted light by making use of reflection from a second glass, we have only to consider *a* as representing the dihedral angle which the plane of incidence of the ray upon that glass forms with the primitive plane of polarization: then the value of F_O will express the intensity and the colour of the reflected ray.

All the particular consequences that may be deduced from these formulas, in giving different values to *i* and *a*, are realized by experiment, as may be seen in the memoirs of which we are giving an account. For example, we may determine all the positions of the plate and of the crystal or glass in which one of the two images disappears. We find, likewise, all those in which the two images can be white, and equal or unequal in intensity, and those in which they are equal in intensity without being white. We see, also, by these formulas, that the plate will not give colours if the incident ray is composed of two white equal pencils, polarized at right angles, or if it is formed of an infinite number of white pencils polarized in every direction like direct light.

Nothing remains indeterminate in these formulas, but the kind of the two colours O and E; or rather of one of them, since both together compose white: but experiment shows that the colour E depends upon the thickness of the plate, and the nature of the substance. By measuring with the greatest exactness the thickness of a great number of plates, by means of a very accurate instrument contrived by M. Cauchois, a skilful optician, M. Biot found that in the same crystal very pure and homogeneous, the thicknesses which polarize such and such a colour are proportional to the thickness of the thin plates of the same substance, which would reflect the same colour in the phenomenon of coloured rings. But Newton has given in his Optics a table of these thicknesses, calculated from very exact experiments. We may then, by means of that table, determine all the colours which will be polarized by the plates of a given crystal, provided we have measured the thickness of a single one, and observed the colour which it polarizes. It is sufficient to refer the thicknesses of these plates to the scale of Newton by the simple rule of proportion. We suppose here that the plates are cut parallel to the axis of crystallization. The factor by which they must be multiplied varies with the nature of the crystal; and even in crystals whose chemical composition is similar, it undergoes sometimes changes, depending upon the contexture of the crystal, and its more or less perfect crystallization. But its value is constant for every homogeneous crystal. In very pure sulphate of lime, of the trapezian variety, the mean value of the factor is about $\frac{1}{9}$; that is to say, that if we express the thickness of the plates in millionth parts of millimetres, and take the $\frac{1}{9}$th of it, the result compared with the third column of the table of Newton will give us the colour E, which each of these plates ought to polarize under a perpendicular incidence. The limits of partial polarization, calculated after this result, for the plates of sulphate of lime, and measured in millimetres, are as follows:

Thickness at which the polarization is not yet sensible: 0·0029548 mm. (0·00011633 inch.), answering to the *very black* of Newton.

Thickness at which the plate polarizes all the incident light: 0·031144 mm. (0·00122614 inch.), white of the first order.

Thickness at which the plate ceases to give colours: 0·45173 mm. (0·01778461 inch.), mixture of all the rings.

We see that it cannot be said that the action of these plates becomes feebler in proportion as their thickness diminishes, since at the thickness of $\frac{3}{100}$ of a millimetre (0·00118 inch.) they polarize the whole incident light; while at the thickness of $\frac{45}{100}$ they polarize only a part. In the first case we have $O = o$. If we place the principal section of the rhomboid in the plane of the polarization of the ray, and turn the axis of the plate in an azimuth of 45°, we shall have $a = o$, $i = 45°$. Then our formulas give $F_o = o$, $F_e = E$. That is to say, that the ordinary image observed across the rhomboid has vanished, and that the extraordinary image contains all the light transmitted. Therefore, when the axis of the plate is placed in an axis of 45°, the pencil which it polarizes has its axes of polarization turned in the azimuth of 90°. We shall see hereafter that this result is general: whatever be the azimuth i, the polarization produced by the plate takes place in the azimuth $2i$. Hence the reason why the separation of the two colours by the rhomboid is most complete in the position $a = o$, $i = 45°$.

The same laws and the same formulas apply generally to plates of mica, and of rock crystal, cut parallel to the axis of the crystals; but the imperfect superposition of the plates of mica produces a greater difference in the thickness of the plates which polarize the same colour, when they are taken from different crystals. The same thing holds with those plates of mica which have no principal sections. Rock crystal presents, likewise, some variations in the analogous thicknesses between one crystal and another; but the relation between the thicknesses and the colours always holds good in homogeneous pieces, when they are split into different fragments. When the rock crystal is very regularly crystallized, the thickness of the plates which polarize the same colour are exactly, or very nearly, the same as in pure sulphate of lime; at least this was the case in the regular pieces of these substances which M. Biot compared.

After having considered the phenomena which take place under a perpendicular incidence, M. Biot examines those which arise when the incidence is oblique. These, while their law remains unknown, appear altogether irregular and anomalous. According as the plate is inclined one way or other, according as its axis is more or less turned, even when the position of the crystal which serves to analyse the light is unchanged, we see the colours of the rays which it polarizes succeed each other apparently without any law. But all these anomalies are only apparent; they assume the character of the most perfect regularity when we observe them with method, and measure them exactly.

But we must here, as we did under the perpendicular incidence, distinguish carefully the intensity and the colour of the pencil which the plates polarize. The intensity follows a law independent of the colour, and the colours a law independent of changes in the intensity.

The fundamental law of the intensities is the following. *If we set out from any position of the plate in which the intensity of the pencil which it polarizes is* o, *or at least in which it is confounded with the primitive polarization, and if, without changing the inclination of that plate, we make it turn round the polarized ray, so that the plane of incidence of the ray upon its surface describes thus an angle* α, *comprehended between* o *and* 90°, *the pencil polarized by the plate will re-appear, and separate from the rest of the transmitted light, in the direction of its polarization; but it will disappear again if, without changing the inclination or the azimuth of the plane of incidence round the polarized ray, we turn the axis of the plate in its plane in such a way as to describe upon the plane an angle* $-\alpha$ *equal and contrary to that which the plane of incidence described.*

This perfect compensation of the two angles thus measured in different planes, is a very singular phenomenon, which we shall find to result from the theory of Biot.

We shall now state the formulas to which this law corresponds. Let A be the dihedral angle which the primitive plane of polarization forms with the plane of incidence of the ray upon the plate. Let i denote the angle which the axis of the plate forms upon its surface with the trace of the plane of incidence, this angle being reckoned in an opposite direction from the preceding. Let O, as formerly, be the intensity of the pencil which preserves its primitive polarization in traversing the plate, E the intensity of the pencil to which it gives a new polarization. Finally, let α be the dihedral angle which the principal section of the rhomboid which serves to analyse the light forms with the primitive plane of polarization.

If we name F_o, F_e, the two ordinary and extraordinary pencils given by the rhomboid, we shall have

$$F_o = O \cos.^2 \alpha + E \cos.^2 [2 (i - A) - \alpha].$$

$$F_e = O \sin.^2 \alpha + E \sin.^2 [2 (i - A) - \alpha].$$

When the incidence is perpendicular $i - A$ becomes the right azimuth of the axis of the plate in relation to the primitive plane of polarization, and we get those formulas which have een formerly stated.

The preceding laws extend likewise to rock crystal cut parallel to the axis of the crystals: but they do not apply to mica; and we shall see afterwards the cause of this exception.

Nothing now remains but to give the manner of finding the two colours O and E, or rather to find one of them, the colour E for example, since the colour O is its complement. The law by which the colours may be found is as follows:—

The inclination of the polarized ray upon the plate being given, and likewise the direction of the plane of incidence, if we turn the plate upon its plane when its axis approaches the plane of incidence, the colours of the ray which it polarizes will appear in the order of the coloured rings, as if the plate became thinner; and on the contrary, when the axis removes from this plane the colours of the extraordinary ray will appear in the order of the rings, as if the plate became thicker. Finally, the colours will become the same as under a perpendicular incidence, whenever the axis of the plate makes with the plane of incidence an angle of 45°.

Thus, calling this last colour E′, and denoting by θ the incidence of the ray, we shall have, under all incidences,

$$E' = E + E\,(A \cos. 2i + B \cos.^2 2i) \sin.^2 \theta$$

A and B being two constant co-efficients. This formula, deduced from experiment, is only an approximation with respect to the incidence θ. It is sufficient for sulphate of lime, where the changes of colour by the variation of incidence are inconsiderable. By studying the same phenomena in other substances, where the changes are much more considerable, M. Biot has discovered another law, much more general, of which this is only a reduction.

The preceding formulas apply likewise to rock crystal, but not to mica. The reason of this is, that the constitution of mica as a crystallized body is different, as we shall see afterwards. In general, when these experiments are repeated, nothing is more striking than the well-defined separation which exists between the laws of the intensities and of the colours. If we calculate beforehand the succession of these colours for all the values of i, from 10° to 10°, that is to say, for all the positions of the axis of the plate upon its plane, we shall perceive, not without surprise, the different colours extend in the different azimuths, in proportion as the values of F_e become null for the different values of i, of α, and of A.

On the 15th June, 1812, M. Biot read to the Class a second memoir, in which he announced that he had found in the polarization of light a new law analogous to the preservation of the *vis viva* in mechanics. That law consists in this, that the colour of the ray polarized by a plate, or by a system of plates whose axes are parallel, depends entirely upon the thickness of the crystallized matter that the light passes through. It is of no consequence in what order the parts of this matter are disposed, nor at what distance they are from each other, provided always that the axes of the plates be parallel to each other. For example, if we take a plate of mica, or of sulphate of lime, which, referred to the table of Newton, polarizes the indigo of the third order; this plate may be mechanically divided into several other thinner ones, which polarize other colours belonging to rings higher up in the table: but when the light passes all these plates in succession, the colour polarized by them all together will always be the indigo of the third order, in what way soever they are placed above each other. M. Biot announced, likewise, that when the axes of the plates were made to cross at right angles, the colour appeared to him to be that which resulted from the *difference* of their thicknesses, instead of from the *sum*. This opinion was afterwards verified, and fully confirmed, by means of a more exact apparatus.

This property constitutes the object of a third *memoir*, read by M. Biot on the 30th of November, 1812: and this memoir itself is only the prelude of a dissertation, in which M. Biot proposes to reduce to mechanical causes, and to one general fact, all the phenomena that he has observed, as well as the formulas that express them.

After having recalled the principal circumstances of these phenomena, and the formulas which he had deduced from them, he shows from these formulas that the plates of sulphate of lime, of mica, and of rock crystal, exposed to a polarized ray with a perpendicular incidence, do not polarize the light upon which they act, according to the direction of their axes, but according to a direction which forms a double angle with the axis of polarization of the incident ray: so that if the azimuth of the axis of the plate, in respect of the plane of polarization, be $2i$, the luminous molecules which the plate polarizes do not turn their axes of polarization in the azimuth i, but in the azimuth $2i$. He shows the constant and unforeseen agreement of this result with the phenomena. This constitutes the first foundation of his theory.

Studying, then, the variations of colours polarized by the plates under different inclinations, he shows that these phenomena seem occasioned by the opposite action of two forces analogous to those which occasion double refraction; with this difference, that of these two forces, which proceed from two rectangular axes, the one tends to increase the polarizing force of the plate, and the other to weaken it: so that by modifying the action of these two axes by inclination, you may make the plate act at pleasure, either as thicker or thinner. Sometimes even a third axis, perpendicular to the plates, joins its action to that of the two preceding; and as it is inclined so as to favour either the one or the other, it increases the action of the plate on the light, or diminishes it, according to regular laws, which may be calculated. These M. Biot has deduced from experiment, and he developes them in detail. This holds with plates of mica regularly crystallized; and this simultaneous action of three axes is the cause of all the anomalies which that substance presents when it is exposed under different incidences to a polarized ray.

To imitate this opposition of two rectangular axes of which these actions are composed, M. Biot, in a fourth memoir, places above each other two plates of sulphate of lime, so that their axes are rectangular, and exposes them in that manner to the polarized ray, beginning at first with very thin plates, and passing successively to those of greater and greater thickness. The colour polarized in this case is always that which agrees with the difference of the thickness; but the variations of these colours by changes of incidence are a great deal more extensive than in simple plates, because they depend upon the sum of the thicknesses.

This result being verified for the whole series of colours contained in the table of Newton, from the smallest degree of thickness in the plates to the greatest capable of producing colour, it was probable that the same property would extend to any thickness whatever. This is in reality the case. If we take two plates of sulphate of lime, whose thicknesses are e' e, and place them above each other in such a manner that their axes cross at right angles, the colour polarized will be that which corresponds to a single plate of the thickness $e' - e$. If the quantity $e' - e$ be within the limits of thickness which give colour, then colours will be produced; but if $e' - e$ be beyond these limits, we shall have two white images. If $e' - e$ is o, the colour polarized by the system is o also, and the second plate destroys what the first produced.

In this manner colours may be produced with plates of any thickness whatever. It is not even necessary that the plates be of the same nature, provided the difference of their actions on light be of the order of that which alone would give coloured images. Thus we may cross a plate of rock crystal with a plate of sulphate of lime, of mica, or of sulphate of barytes; but the thicknesses which must be given to each of these crystals are different, according to the intensity of their action. A plate of sulphate of lime, of a millimetre in thickness, is sufficient to produce colours with a plate of ice several centimetres thick.* We have only to cross their axes at right angles. This takes place equally, whether the plates touch, or be separated to some distance from each other.

* A millimetre is 0·03937 inch, a centimetre 0·3937 inch.

The experiments contained in the first memoir of M. Biot proved that the thicknesses of the plates which polarize such or such a colour have a constant ratio with the thin plates which reflect the same colours in the coloured rings. From the new phenomena which we have stated it is evident that this property is not confined to thin plates; but that it extends to every distance across the thickness of bodies. This is the second fact which serves as a basis to the theory of M. Biot.

He explained this theory in a fifth memoir read to the Class on the 7th of December, 1812. "I do not propose," says M. Biot, "to seek an hypothesis to explain the facts which I have observed, I wish only to compare them together, and to reduce them by mathematical consideration to a single fact, which will be the abridged expression of them, and from which we may afterwards draw by calculation, not only the phenomena which I have noticed, but all those which may result from their combination."

This general property, which includes all the others, is the following. Let us suppose that a plate of sulphate of lime, of mica, or of rock crystal, cut parallel to the axis, is exposed perpendicularly to a polarized ray, so that its axis of crystallization makes an angle i with the plane of the polarization of the ray; the molecules of light, in falling upon that plane, will penetrate at first to a small depth without undergoing changes in their polarization; but at a certain limit, different for the molecules of different colours, they will begin to oscillate, like magnetic needles, round their centre of gravity. The magnitude of these oscillations, which will be o and $2i$, will bring by turns their axes of polarization into the azimuths o and $2i$; but as the celerity of the oscillations is not the same for molecules of different colours, it follows that they do not all arrive at the same time at these two limits, and this occasions the difference of colour which we observe in them. Finally, the inequalities of their celerity mixing more and more with each other, they will at last form two white pencils, situated in the same straight line, one of which will have its axis of polarization turned in the azimuth $2i$, while the other will be in the azimuth o, so that the last will appear to have preserved its primitive polarization. M. Biot determines the rapidity of these oscillations for the different molecules of light. He fixes the depth at which they commence, and determines generally all their laws. He even calculates that of the force which produces them, and shows from the phenomena that it is proportional to the angle formed at each instant by the axis of polarization of the luminous molecules, and the axis of the crystalline plate: and as the time of these oscillations may be calculated from the thickness which the light traverses during them, there results a relation between the force which produced them and the size of the particles of light on which it acts, just as the duration of the oscillations of a pendulum gives a relation between its length and the intensity of gravity.

Having arrived at this general result M. Biot shows, *a posteriori*, that it really includes the two laws which he employed in order to establish it; for he shows that we may draw from it the very same formulas which he had obtained at first from the experiments contained in his first memoir. He devotes the rest of his paper to show how, from the same principle, we may calculate and foretell all the other phenomena of polarization which are exhibited by plates of sulphate of lime, of mica, and of rock crystal, cut in any direction whatever, and exposed in any way to rays polarized, either by refraction or reflection; but these ulterior applications, though already calculated by M. Biot, being the object of different memoirs which have not yet been read to the Class, we cannot give an account of them here.

Memoirs on different new Phenomena of Optics. By M. Arago.

We could have wished to have given as detailed an account of the different memoirs in which M. Arago has exposed to the Class his new experiments on light. We should have seen experiments not less interesting, and theoretic ideas, which, to be rendered sufficiently clear, require new experiments, of which M. Arago has conceived the idea, and formed the plan; but being able to devote to these experiments only the few moments of leisure left him by his functions of Astronomer at the Imperial Observatory, he has not been able to communicate his ideas to the Class, but in proportion as he could notify them in detached notices. He proposes to complete them, and to class them in a luminous order. Thus we are obliged to defer to our history for 1813, our detail of these experiments, made in order to throw light on the most difficult points of optics, that is to say, the explanation of the phenomena of the colours of bodies.

Different Memoirs. By M. Rochon.

M. Rochon, while communicating to the Class some new researches in which he was engaged in the year 1812, had occasion to notice some of his old labours, either little known, or intimately connected with his recent speculations. In a memoir on the art of multiplying copies he has noticed the process of the celebrated Franklin, who first introduced into France the art of multiplying copies of writings. M. Rochon at that time improved these processes, by contriving a machine for engraving, which was approved of by the Academy of Sciences. In continuing the subject, he proved by his experiments that the ancient bronze dies, composed of copper hardened by tin, must have been formed by a method similar to that at present known by the name of *cliché* (by letting a heavy weight fall on them). He mentions, likewise, other processes, which show the part he took in the progress of the art of multiplying copies.

In a second memoir he has explained the construction of a prismatic micrometer, for measuring exactly the diameters of the sun and moon. The object glass of this micrometer is composed of rock crystal and flint glass; it is at the same time achromatic, and gives a refraction of 26′. This is not sufficient for measuring, which would require a refraction at least of 32′. But an achromatic prism of crystal, similar to that in ordinary micrometers, moving along the axis, serves to complete the measurement, though it has only a double refraction of 6′, because we may chuse a time when the apparent diameters are only 30′ or 31′. The advantage of this construction would be that in a glass of this nature, supposing only 2 metres (78·742 inches) of focal length, each second will occupy the space of 3 millimetres (0·11811 inch). Hence it will not be difficult to estimate the tenth of a second. Before this improvement of his micrometer, M. Rochon had measured the diameters of the smallest planets; and from the calculation of his observations we have in general found that something ought to be subtracted from the diameters commonly adopted by astronomers. It will be curious to see whether from the measurements which M. Rochon will take of the sun and moon with his new instrument, we shall have a similar diminution to make in the diameter of these luminaries, which are of much greater importance for practical astronomy, and which in fact have always been found smaller by some seconds in proportion as the instruments became more perfect.

In a third memoir M. Rochon has given the general theory of instruments serving to measure angles, either by mirrors, or by achromatic prisms of glass or rock crystal.

The fourth memoir contains an account of the employment of metallic wire-work to render buildings incombustible. He relates an experiment made by M. Dyle, who has covered with his cement, impenetrable to water, six metres square of this wire-work.

In the fifth memoir, on printing, after having spoken of the origin and progress of this art, he points out the method of making a small number of characters serve for printing a large work, particularly for printing tables of logarithms.

The sixth memoir was destined to point out the advantages of employing mica, commonly called Muscovy glass, for letting in the light.

On the 22d of June M. Rochon presented to the Class an instrument which, by means of a formula of M. le Comte Lagrange, reduces quickly an apparent distance of the moon from the sun to the true distance. A prism of rock crystal gives the double image of the moon under a constant angle of 30′. By the circular motion of this prism behind the transparent part of a small mirror, we obtain the correction of the combined effect of parallax and refraction. A simple proportion is sufficient when we have observed two distances of the moon from the sun, and when in that double operation we have brought in contact the two images of the moon successively: the difference between these two distances compared with that of 30′ will give that which results from the variable effect of refraction and parallax. The method is very ingenious. Experience must be consulted to determine its precision.

In a seventh memoir M. Rochon has given a new process to know by the mean height of a man the distance at which that man is from the eye of the observer. It is obvious that this method cannot be rigorous, but it may have its utility in the operations of military tactics. The instrument is very simple. No use is made in it of the contact of two images. It is sufficient that the feet of the first image appear in the same horizontal line with the head of the second, of which one can judge almost as exactly as of contact.

The last memoir of M. Rochon has for its object to render sea water potable. He had directed his attention to this object many years ago, but had suspended his experiments when he understood that Meusnier was engaged in solving the problem. The method of M. Rochon consists in a large alembic, in which an imperfect vacuum may be formed, sufficient to make water boil at 50° Reaumur (144·5° Fah.).

From the preceding statement it is obvious that M. Rochon endeavours every way to be useful, and that his zeal suggests to him resources as varied as the objects to which he applies them.

Astronomy.

M. Bouvard announced to the Class on the 3d of August the discovery, which he had made on the 1st of that month, of a small comet in the constellation Lynx. At the end of the meeting a letter was received from M. Blanfrain, Director of the Observatory of Marseilles, communicating three observations on the same comet, which had been observed at Marseilles ten days sooner by M. Pons, Keeper of the Observatory. This comet was small. The observations, at least at Paris, have been few and difficult. M. M. Bouvard and Nicolet have calculated its orbit, which does not resemble that of any of those before known.

Astronomers neglect no occasion to complete the tables in which all these orbits are united. M. Lindenau has very lately published a supplement to all these tables. We find in it 40 orbits newly determined; 22 by Burckhardt, among which is not the comet of 1695, the orbit of which he has recently calculated from observations found in manuscript in the Observatory. The same astronomer has given observations on the motions of the stars in Cassiopeia.

Tables of the Moon. By M. BURCKHARDT.

We formerly announced these tables as about to appear, and we have nothing to alter in the notice which we gave, in the history of the Institute for 1811. But we can at present add some details respecting their composition, and their relative degree of precision. A commission, named by the Board of Longitude, was charged to examine them. It was decided that they should choose a great number of observations distributed in different points of the lunar orbit, and that they should be calculated according to the tables of Burg and of Burckhardt, and that each of the calculations should be made twice over, to prevent error. To compare the difference between the calculations and observations, it was resolved to employ the method of the smaller squares, as that which would furnish the most probable result. For the longitude the sum of the squares was 70083″ for M. Burg, and only 4602″ for M. Burckhardt. The correction of the epoch for the middle of the year 1804 was 0·2″ and 0·1″, that is to say, insensible. These results were from 167 observations, made at Greenwich and at the Imperial Observatory. By 137 observations, made at the Imperial Observatory, and at that of the military school, the sums of the squares were 6439″ and 4182″. The correction of mean longitudes for the middle of 1811 was 1·4″ and 0·1″.

For the latitude the same method proved that the new tables had an advantage almost equal over those of M. Burg.

The tables of the two authors compared with the observations of Delahire and Flamsteed indicated for M. Burckhardt a real though smaller advantage.

M. Burg had introduced a new equation at a long period; but between two arguments, almost equally probable, upon which that equation might depend, M. Burg had decided for one. After mature reflections, and by the advice of M. Laplace, who had discovered this equation, M. Burckhardt decided in favour of the second. These tables contain 32 equations of longitude which depend only upon mean arguments, and 4 which depend upon arguments successively corrected from all the preceding equations. The tables of parallax are calculated solely from the theory of M. Laplace.

(*To be continued.*)

Article XI.

METEOROLOGICAL JOURNAL.

1812.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap	Rain.	
12th Mo.										
Dec. 25		30·46	30·40	30·430	35	31	33·0	—		☽
26		30·50	30·40	30·450	37	30	33·5	—		
27		30·52	30·48	30·500	36	29	32·5	—	1	
28		30·52	30·32	30·420	43	32	37·5	—		
29		30·32	30·15	30·235	46	42	44·0	—		
30		30·15	29·92	30·035	50	42	46·0	—		
31		29·81	29·75	29·780	44	40	42·0	—		
1813.										
1st Mo.										
Jan. 1	W	30·09	29·81	29·950	45	38	41·5	—		
2	S W	30·26	30·09	30·175	44	36	40·0	6		●
3	W	30·30	30·26	30·280	41	34	37·5	—		
4	S E	30·30	30·09	30·195	42	34	38·0	—		
5	S W	30·09	29·86	29·975	44	37	40·5	—	5	
6	S W	29·77	29·70	29·735	50	40	45·0	—	9	
7	N W	29·70	29·30	29·500	46	40	43·0	—	11	
8	S W	29·62	29·30	29·460	48	28	38·0	—		
9	N W	29·87	29·75	29·810	41	31	36·0	9	9	☾
10	N W	29·82	29·70	29·760	34	28	31·0	—		
11	S E	29·80	29·70	29·750	40	26	33·0	—		
12	S E	29·70	29·61	29·655	34	29	31·5	—		
13	S E	29·58	29·53	29·555	38	34	36·0	5	16	
14	N E	29·74	29·53	29·635	38	33	35·5	—		
15	N W	30·00	29·74	29·870	38	28	33·0	—		
16	S W	30·20	30·00	30·100	44	29	36·5	—		○
17	S E	30·20	30·04	30·120	35	28	31·5	—		
18	S E	30·14	30·04	30·090	31	30	30·5	—		
19	E	30·26	30·14	30·200	33	31	32·0	—		
20	E	30·27	30·26	30·265	34	30	32·0	—		
21	N E	30·35	30·27	30·310	34	29	31·5	—		
22	E	30·50	30·35	30·425	36	23	29·5	·15		
		30·52	29·30	30·022	50	23	36·25	0·35	0·51	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

1812. *Twelfth Month.* 25. A very slight sprinkling of snow. 27. A little snow has fallen in the night. 30, 31. Windy nights: small rain at intervals.

1813. *First Month.* 1. Small rain at intervals. 3. Misty morning. 5. Windy. 6. The same: small rain. 7. Very misty, a. m. dark and cloudy, p. m. About 8, some lightning, which was soon followed by a shower. 9. Hoar frost: at 9, a. m. thick air, with *Cirrostratus* and *Cirrocumulus:* sounds come freely with the wind, which is now S.S.W. Sleet and rain followed this observation within an hour. 13. a. m. Overcast: sleet and rain. 14. Cloudy. 19. a. m. A little snow.

RESULTS.

Winds Westerly during the greater part of the period: afterwards Easterly.

Barometer: highest observation ......30·52 inches;
Lowest ....29·30 inches;
Mean of the period....30·022 inches.

Thermometer: greatest height ....50°
Least .... 23°
Mean of the period .... 36·25°

Evaporation, 0·35 inches. Rain, 0·51 inches.

*** After the next period these observations will be continued at Tottenham, Middlesex, to which place the observer has removed his residence.

Tottenham, L. HOWARD.
Second Month, 24, 1813.

Erratum in last Number.

Page 148, line 6 from bottom, *for* "executors," *read* "manuscripts."

Article XII.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap.	Rain.	
1st Mo.										
Jan. 24	N E	30·46	30·37	30·415	37	24	30·5			☾
25	N E	30·47	30·45	30·460	36	29	32·5			
26	N E	30·48	30·40	30·440	41	35	38·0			
27	N	30·49	30·47	30·480	39	21	30·0			
28	Var.	30·39	30·37	30·380	32	20	26·0			
29	Var.	30·48	30·39	30·435	34	21	26·5			
30	N W	30·48	30·44	30·460	42	30	36·0			
31	N W	30·50	30·44	30·470	48	34	41·0		0·27	
2d Mo.										
Feb. 1	Var.	30·33	30·24	30·285	41	30	35·5			●
2	N W	30·37	30·32	30·245	41	36	38·5			
3	N W	30·45	30·37	30·410	43	34	38·5			
4	W	30·45	30·29	30·370	41	34	37·5			
5	S	30·29	29·78	30·035	47	36	41·5		—	
6	S W	29·89	29·78	29·835	47	38	42·5			
7	S W	29·98	29·79	29·885	48	37	42·5		—	
8	S W	29·66	29·63	29·645	52	44	48·0		—	○
9	S W	29·88	29·66	29·770	51	35	43·0		0·36	
10	W	30·00	29·88	29·940	46	33	39·5			
11	S	30·00	29·75	29·874	47	35	41·0			
12	S	29·75	29·28	29·515	56	44	50·0		—	
13	S W	29·48	29·37	29·425	57	39	48·0		0·33	
14	S W	29·38	29·27	29·325	52	42	47·0		0·30	
15	S W	29·34	29·27	29·305	52	41	46·5		0·18	☾
16	S W	29·44	29·34	29·390	48	41	44·5		—	
17	S W	29·37	29·30	29·335	52	43	47·5		0·27	
18	S W	29·88	29·37	29·625	52	41	46·5		—	
19	S	29·66	29·60	29·630	56	40	48·0		0·19	
20	S W	29·80	29·66	29·730	53	42	47·5		—	
21	S W	29·70	29·69	29·695	57	49	53·0			
		30·50	29·27	29·957	57	20	40.58		1·90	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

The Binder will place this Leaf to follow Page 240.

REMARKS.

1813. *First Month.* 24. Light clouds and sunshine. 28. Rime on the trees: very misty a. m. clear p. m. 29. Hoar frost: the sky overcast. 30. Misty to the S. a. m. A grey day. 31. Misty a. m. Heavy *Cirrostratus* clouds: rain at night.

Second Month. 1. The *Cumulostratus*, which has not for a long time been exhibited, appeared to-day in large masses. 7. Showers and wind: at sun-set, several large clouds of the modification *Nimbus*. 8. Stormy. 9. A violent thunder gust from the west about 2 p. m. by which considerable damage was done to the roofs and chimnies of houses, &c. This was followed by a series of heavy gales (continuing with a few short intervals of calm and pleasant weather) to the end of the period. The *lunar halo* appeared before several of these, of a large diameter; and, on the 18th, about 11 a. m. there was a brilliant rainbow. The river Lea has considerably inundated the adjacent lands.

RESULTS.

Winds, in the fore part, northerly, with a very dry, dense air, and low temperature: in the latter part, southerly, with a rare and moist atmosphere, and high temperature.

Barometer: greatest observed elevation, 30·50 inches;
Least ....29·27 inches;
Mean of the period ......29·957 inches.

Thermometer: highest ....57°
Lowest ....20°
Mean of the period....40·58°

Rain, 1·90 inches.

The account of evaporation has been again interrupted, and is therefore omitted, in order to be resumed in next report.

TOTTENHAM, L. HOWARD.
Second Month, 23, 1813.

ANNALS

OF

PHILOSOPHY.

APRIL, 1813.

Article I.

Some Observations in Answer to Mr. Chenevix's Attack upon Werner's Mineralogical Method. By Thomas Thomson, M.D. F.R.S.

The paper on which I propose to make some observations was first published in the 65th volume of the *Annales de Chimie*, rather more than five years ago. It was afterwards animadverted upon, in the 69th volume of the same work, by M. D'Aubuisson. Had it continued in its original French dress I should have been disposed to consider it as a sacrifice made by Mr. Chenevix upon the shrine of French vanity; intended, perhaps, to render his situation more agreeable during his abode in Paris, and to facilitate his return to his own country: but as it has been no less than twice translated into English, and as Mr. Chenevix has himself added to one of the translations what he calls an answer to D'Aubuisson, some notice of it seems necessary. From the confident style in which it is written, and the spirit of raillery which runs through it, the paper is calculated to produce a considerable effect upon young men just beginning the study of mineralogy. On that account I hope it will not be considered as out of season to make a few strictures on it, even at this late period.

It was probably the respect to which they thought Mr. Chenevix entitled that has induced British mineralogists to take no notice of his essay. For my part I think they have reasoned inaccurately. The only true way of treating a man of science with respect, is to examine the arguments by which he supports his opinions, to adopt them if they appear sound and conclusive, but to refute them if they are erroneous. This is the course which I mean to take. If I shall be so fortunate as to place the subject in its proper point of view, I trust I shall promote the cause of science, and make some compensation to the feelings of a nation, which Mr. Chenevix appears to me to have treated with harshness and injustice.

I have a very high opinion of Mr. Chenevix, both as a chemist, and as a man of talents, and regret exceedingly that he seems disposed to abandon that career of experimental investigation in which he was proceeding with so much credit to himself, and benefit to the science to which he seemed so devoted. I hope this change of taste was not occasioned by the mistake into which he fell respecting palladium. From the very nature of chemistry, mistakes must now and then occur, notwithstanding every possible caution. Nor do I recollect any chemist who has not occasionally fallen into them, except perhaps Dr. Black and Mr. Cavendish. It is not the unavoidable errors, which those who cultivate this delightful but intricate science must occasionally commit, that have such a tendency to injure their credit as philosophers, as the indulgence of that supercilious or rather ludicrous style, which is suited only to comedy and burlesque, and not at all adapted to the gravity of science. I do not know what effect Mr. Chenevix's attack upon the Wernerian method of mineralogy produced upon others; but, for my own part, I must acknowledge that it was not Werner and the Wernerian method which sunk in my estimation, but the author of the *Reflections*: and this effect was increased tenfold by Mr. Chenevix's appendix to the English translation of his essay.

It may be necessary to premise in the first place, for the sake of the general reader, that there are two systems which at present divide the mineralogical world; namely, the system of Werner, Professor of Mineralogy at Freyberg in Saxony, which has been generally embraced in almost every part of Europe and America; and the system of the Abbé Haüy, which has been adopted, though not universally, by the French, and I believe also by several British mineralogists. Werner may be considered as the founder of mineralogy as a science; for before his time it was not entitled to the name. Haüy came later; and was assisted in his system by the theory of Bergman, and the previous labours of Romé de Lisle.

The object of Mr. Chenevix's reflections is to prove that the system of Werner is destitute of all merit; that it teems with absurdities and contradictions, and is unworthy of the attention of a man of science: while the system of Haüy is excellent in every respect, simple, exact, and truly scientific. Not satisfied with this sweeping preference, he attacks the Germans as a nation, and will not allow them to possess the smallest merit either in literature or science. Though chemistry and mineralogy have been much more generally cultivated in Germany than in any other country, and though during the last 35 years they have undergone a complete revolution, he affirms that not a single philosophical idea has been furnished by a German, while the whole philosophy of chemistry and mineralogy is due to the French.

The *Reflections* of Mr. Chenevix consist of three distinct parts: 1. A discussion respecting mineral species, in which the *absurdity* of the Wernerian method is exposed, and the immense superiority of the system of Haüy stated and descanted on. 2. An examination of the descriptive language of Werner, in which its defects, inaccuracies, and inelegance, are enlarged upon, and exposed to ridicule. Mr. Chenevix allows that it answers well enough to distinguish minerals from each other; but he contends that it is too easy, and too unphilosophical, to be adopted by a man of science, and that it is fit only for miners, and persons destitute of education. 3. A critique, or rather an invective, against the German mode of teaching, German literature, and German literati; and a comparison of the state of philosophy in Britain, France, and Germany.—Let us take a view of each of these topics in the order presented by Mr. Chenevix.

I. It is somewhat singular that though Mr. Chenevix studied mineralogy, as he informs us, for 18 months at Freyberg, and of course must have heard the principles of the Wernerian method detailed by Werner himself, yet all the knowledge of it, which he seems to possess, he obtained from the writings of D'Aubuisson and Brochant, two French mineralogists; the first of whom wrote the essay, which Chenevix quotes, while he was attending Werner for the first time; and Brochant published his system of mineralogy without having had the previous advantage of studying at Freyberg at all. Mr. Chenevix quotes the following passage from D'Aubuisson, on which he builds a great part of his reasoning:—

"All minerals which have essentially the same constituent parts, both in respect of quality and quantity, form the same species; and all those which differ essentially belong to different species. If different minerals in the same species having the same characters respectively (one character excepted), differ from other minerals by two or three characters (a greater number would occasion a difference in species), they form a distinct subspecies. When an individual in a species or subspecies has only one character different, it forms a variety."

On this quotation Mr. Chenevix descants at considerable length. He shows that it is improper to assume the number of different characters as the base of classification, without any regard to the *value* of these characters: he points out the inconveniences and absurdities that result from Werner having divided the external characters into genera and species; and shows that in the formation of species Werner himself has not followed his own principles; but has been guided entirely by caprice; that his species are founded upon no principle whatever, and are often, therefore, erroneous and improper. The whole of these animadversions, which occupy a considerable space in Mr. Chenevix's paper, are founded upon two mistakes, into which he has fallen, very difficult to be accounted for.

That a man of abilities should have studied mineralogy with assiduity for 18 months, and not have acquired the knowledge of the first principles of the science, is as extraordinary a fact as any which I have ever met with; for I will not suppose that Mr. Chevenix was so uncandid as to mis-state, on purpose, the first principles of the Wernerian classification, that he might have it in his power to point out its supposed absurdities, and triumph in his own superior acuteness. This could not promote the cause which he has espoused with such zeal. Every tyro in mineralogy could expose his reasoning at pleasure; and Mr. Chevenix would have earned a character the most disgraceful to a man of science—the want of honesty and of candour. I am rather disposed to ascribe the whole of his reasoning to ignorance; but in that case he was the most improper person possible to write on the subject, and could not be a judge of the merits of a method with which he was utterly unacquainted.

It is not true that Werner makes the *number* of different characters the base of his arrangement. *It is not true* that Werner divides his external characters into *genera and species*, which influence his arrangement. The first proposition, indeed, might be drawn from the paragraph of D'Aubuisson above quoted; but it never was taught by Werner, nor by any of his disciples, and is not to be found in any of the numerous works on Wernerian mineralogy, which have made their appearance in every country of Europe. D'Aubuisson himself has acknowledged the inaccuracy of his assertion, and has formally denied that Werner ever taught it. With respect to the second assertion, that Werner divides his external characters into genera and species, which influence his classification; this is so far from being the case, that Werner, in his *Treatise on the External Characters*, expressly finds fault with his predecessors for having done so, and points out the impossibility of making any such division the base of a classification of minerals.*

* Mr. Chevenix, in his answer to D'Aubuisson, published as an appendix to the English translation of his paper, still continues to affirm that Werner has divided his external characters into genera and species which influence his arrangement, and he quotes a passage from Weaver's translation of Werner's

Let us now consider the mineral species a little more closely, that we may be able to determine whether there be any foundation for Chenevix's assertion that Werner follows no principle whatever, and that the principle of Haüy alone is precise and scientific. Werner's definition of a species is this: "All minerals composed of the same constituents combined in the same proportion belong to the same species, while minerals composed of different constituents belong to distinct species." Haüy's definition of species is this: "Minerals composed of the same constituents, and having integrant molecules of the same form, belong to the same species." I appeal to Mr. Chenevix himself which of these two definitions is most scientific. Werner's species depend alone upon the constitution of minerals, which is surely the essential circumstance: for minerals are not organised beings, like animals and plants, capable of being classed according to the form of their organs respectively. The circumstance in minerals, which is the most interesting to us, and upon which, as far as we have any means of judging, their properties must depend, is their composition. Upon it, therefore, it seems most proper that their division into species should

book in support of his assertion. The passage in question, had it been *fairly* quoted, would have proved directly the reverse. To convince the reader of this, I shall insert here the whole paragraph, translated as literally as possible from the original German. The paragraph is needlessly diffuse, which gives it a clumsy appearance; but the meaning is sufficiently obvious.

"The best mode of exhibiting the mutual relations of the external characters is to reduce them into a regular system; that is, to divide them into genera and species, and then to arrange them according to their natural succession. Genera of external characters, or generic characters, are those which show what is to be determined in a mineral; such are colour, coherence of the particles, weight, taste, &c. Thus, for example, when I say copper pyrites has a colour, I have as yet determined nothing. I have only shown what is to be determined. These generic characters are again *general* and *particular*; the *general* are those which show what is to be determined in all minerals; the *particular*, what is to be determined in a part of fossils. The above cited example illustrates the general character; as *solidity*, external shape, lustre, &c. do the particular, as they occur only in solid minerals. The *species* of external characters, or *specific characters*, are those which determine what we have to say of a mineral in regard to a generic character. As for example, when I say of copper pyrites, in regard to colour, it is *yellow*; or in regard to hardness, that it is *semihard*—these specific characters afford us a conception of the exterior of the fossil, or the description of its exterior; but the generic characters serve only to reduce the specific characters in the system under genera. Lastly, the *varieties* are those characters by which we determine with accuracy what is to be observed in a mineral in regard to a specific character. As for example, when I say of copper pyrites that it is *gold yellow*, or of the diamond in respect to hardness that it is *uncommonly hard*. As these specific characters vary very much, and as many minerals are often distinguished by one variety, it is necessary to determine, with as much accuracy as possible, in regard to one or other of the common characters, not only by the specific characters, but also by the varieties." *Werner's von den ausserlichen Kennzeuchen der Fossilien*, p. 65. A writer must be greatly at a loss for objects of accusation, when he has recourse to misquotations and false constructions to establish his positions.

depend. So far I believe all are agreed. Even Mr. Chenevix will not condemn Werner's definition in the abstract, though he has not thought proper to take any notice of it, for what reason he himself is best able to inform us.

But Haüy's definition, it seems, is infinitely preferable. Why so? Haüy's definition is the same as Werner's, only he adds a particular of which Werner has taken no notice. Minerals, to belong to the same species, must not only agree in their composition, but also in the form of their integrant molecules. The superiority of the definition must depend upon this addition. Now the *cleavage*, for the form of the integrant molecule is neither more nor less than a mathematical hypothesis adopted by Haüy, constitutes a character of minerals, which I admit to be of the very first importance; but so is the *specific gravity*. And I will appeal to Mr. Chenevix himself, whether the specific gravity is not as important a character as the cleavage; for whenever minerals differ much in their specific gravity, Haüy always places them in distinct species, even though the form of their integrant molecules be in his opinion the same. Haüy might, therefore, with as much propriety, have added to Werner's definition of species, that minerals belonging to the *same species* must have the *same specific gravity*, as the same form of integrant molecule. And several other characters stand in the same situation, and have just as fair a claim to be introduced into the definition of species as the cleavage.

Thus with respect to Haüy's definition of mineral species, so far from considering it as superior to the definition of Werner, I think it inferior, in consequence of the arbitrary addition with which it is clogged. But even supposing the addition of Haüy to be perfectly correct in itself, and supposing it possible to determine the angles of crystals with perfect accuracy, still the definition would be improper as the basis of a system of mineralogy, because it would exclude from the consideration of mineralogists all those numerous minerals which are not crystallized, and with respect to which therefore it is impossible to determine their primitive form. Now this is the case with by far the greatest part of minerals. What can be more preposterous than a definition which excludes from the rank of species the greater number of the very substances which it is its professed object to classify? Mr. Chenevix contends that this is perfectly proper, and insists upon it that all uncrystallized minerals should be excluded. That is, that 99 hundredth parts of all the minerals in nature should be discarded from the consideration of the mineralogist, because they happen to be inconsistent with an arbitrary definition. But Haüy has conducted himself with more moderation. He has introduced into his system two mineral species, neither of which contains a single crystallized specimen; I mean *chromate of iron* and *cerite*. Nay, in his last publication, he allows that the knowledge of the integrant molecule is not necessary to enable us to form and describe species, and has thus brought back his own definition, so highly prized by Chenevix, to a state of identity with that of Werner, which Chenevix has so unmercifully belaboured, as utterly destitute of all pretensions to science or common sense.

Thus with respect to the abstract definition of mineral species, the superiority is unquestionably due to Werner. Many mineral species have been established in exact conformity with this definition. *Cinnabar, pyrites, tungstate of lime, sulphate of strontian*, may be mentioned as well-known examples. But I must acknowledge that a rigid application of the definition of mineral species is impossible; otherwise the number of species would become almost infinite, and a system of mineralogy of no use whatever. The whole globe of the earth is composed of minerals; and the constituents of these minerals, as far as they have been examined, consist of no less than 50 distinct substances combined in a great variety of ways. Minerals were early divided into four classes, which possess characters sufficiently distinct from each other; namely, *stones, salts, combustibles*, and *ores*. In the *salts*, and a good many of the *ores*, the composition is perfectly fixed, and the minerals belonging to them may be arranged in strict conformity with the Wernerian definition. The combustible minerals are few, and easily arranged; though the science of chemistry is not yet far enough advanced to allow us to determine with precision respecting their composition. The stones are composed of the nine earths, water, the fixed alkalies, and about seven metals in the state of oxides; and these bodies are found united in an almost infinite number of proportions. Were the definition to be strictly adhered to, almost every specimen of stone would constitute a distinct species; and there would be as many species as mineral masses. Some means must be devised to prevent this enormous multiplicity, otherwise mineralogy would be of no service to mankind. The method followed by the older mineralogists was to pitch upon some one quality, which they considered as of primary importance: all minerals agreeing in this quality they referred to the same species; all disagreeing, to a different species. Haüy has followed the plan of the older mineralogists in this respect. He has pitched upon the form of the integrant molecule, and has laid it down as a rule that all minerals having the same integrant molecule (provided the chemical composition agrees) belong to the same species.

The rule laid down by Werner is very different. He conceived it necessary to include in his system all the minerals in nature, though he believed at the same time that a great many of them had not been arranged by nature into species. It was necessary, however, to divide them into species, for the sake of mineralogy. Such species are merely conventional and artificial, and the basis of their formation is utility. Some rule must be followed in forming them. Now Werner's mode is this: and I confess that to me at least it appears excellent. He examined all the external characters of minerals, and divided them under various heads. Some of these characters are susceptible of a great number of varieties: *colour*, for example. He determined all the different shades of colour which occur in the mineral kingdom, and drew them up into a catalogue, in the order in which the different shades run into one another. The same was done with respect to *hardness*, *specific gravity*, and every other character susceptible of variation. A certain range of each of these characters was allowed to minerals belonging to the same species. Thus two points were taken in the catalogue of *colours*, between which all the colours belonging to a given species would be found. In like manner two points were taken in the catalogue of *hardness*, *specific gravity*, &c.: and between these two points lie the hardness, specific gravity, &c. of every mineral belonging to a given species.

Thus Werner formed all the characters susceptible of it into suites, gradually passing through a variety of changes from one extremity of the suite to the other. Certain points were fixed upon at some distance from each other in each of these suites, and all the minerals possessed of the characters lying between these two points were considered as belonging to the same species. Thus with respect to colour, for example, it is not necessary for all minerals belonging to the same species to have the same colour; but they must possess some one of the colours lying between the two points in the series of colours pitched upon as including all the colours belonging to that species. Hence the colour suite between these two points characterizes the particular species, and becomes as important and unvarying a character as any other character whatever. The same rule is followed with respect to the hardness, the specific gravity, the crystals, and all the other characters. So that the rule for forming species does not require perfect identity in all the characters; but a certain variation is allowed in each; and provided this variation keeps within the prescribed limits, it does not prevent minerals from being classed together. Exact identity is not insisted on; but the minerals classed together under the same specific name approach as nearly as possible to each other in their properties. To form species according to this rule is by no means an easy task. It requires, in the first place, a collection of a considerable number of specimens, and a very careful examination and comparison of all the characters. The extensiveness of the suites which constitute particular characters (as *colour*) will necessarily vary in different species; because it will depend in some measure on the permanency of the other characters. In some species the colour suite is very extensive; because the other characters may nearly coincide in a great variety of specimens, which, notwithstanding, may differ in their colours. The same observation may be applied to the other characters. But after a species is once defined or described (for the description is the only possible definition of it), then every suite of characters becomes just as characteristic of the species as if it were not susceptible of the least variation.

Such is the method which Werner has prescribed to himself in forming species. And I appeal to Mr. Chenevix himself whether it is not the best, or rather whether it is not the only method possible; provided it be true that most stony bodies are not in fact divided into species by nature, but that the division of them into species is entirely conventional and artificial. This, however, is a conclusion which I am sensible Mr. Chenevix will not readily admit; for he affirms that all minerals which deserve a place in a mineral system are divided into species by nature. This also must be the opinion of Haüy. It is an opinion which might be plausibly supported if we were to adopt their method of proceeding, that is to say, reject $\frac{19}{20}$ths of the whole mineral kingdom as unworthy of notice, and make our selection ad libitum out of the remaining 20th. But it is an opinion which every person, who conceives mineralogy intended to convey information respecting minerals in general, and not confined to small spots and corners of the system, will find it impossible to establish.

That we may be enabled to form some kind of judgment, let us compare the definition of Haüy with the minerals as he has arranged them into species. That definition consists of two parts: let us consider them in succession. 1. As to the first part, it is obvious that, as far as chemical analysis goes, there does not exist any identity in the composition of those minerals which he places under the same species. Thus in three specimens of *mica* analysed by Klaproth the proportions of alumina were 20, 34, 12, respectively; the proportions of iron, 15, 5, 22; and those of potash, 15, 9, 10. One of the species contained 9 per cent. of magnesia, while the other two exhibited no trace of that earth. But it is needless to multiply examples: the fact is universally known and acknowledged. I refer Mr. Chenevix to the various analyses of *felspar* by Vauquelin, Klaproth, and others; to his own analyses of sapphire compared with those of Klaproth; and, indeed, to almost all the analyses of stony bodies hitherto published. Mr. Chenevix, indeed, admits this perpetual diversity; but he has fallen upon a method of accounting for it without being under the necessity of giving up his opinion. When minerals crystallized, says he after Dolomieu, they were enveloped in foreign bodies. Hence they usually contain a variety of foreign matter mechanically mixed. Now, as chemistry is not in possession of a method of distinguishing between those ingredients which are chemically combined, and those which are mechanically mixed, both together are stated as belonging to the constituents of the mineral; and hence a variation is exhibited in its constitution, which would disappear if we could get rid of all the ingredients that are only mechanically mixed, and of course in reality foreign. Were we to grant the truth of this explanation, still it would follow that the composition of stones is insufficient to enable us to arrange them into species, since by the confession of Mr. Chenevix himself we have no means of discovering what that composition is. Thus it is admitted by Mr. Chenevix that chemical analysis (in the present state of the art) is not sufficient to enable us to divide all minerals into species.

I cannot avoid noticing here Mr. Chenevix's remarks regarding Werner's conduct with respect to chemical analysis. When it agrees with his previous classification, says Chenevix, he allows it to be of weight; but, when it disagrees with it, he discards it altogether, and will not allow it to be a sufficient reason for altering his arrangement. This accusation must be taken with some limitation. Werner has been induced in some cases, by chemical analysis, to separate into two species minerals which had been formerly considered as constituting only one. Mr Chenevix himself acknowledges that this was his reason for separating *sulphate of strontian from sulphate of barytes*. But when the results of chemical analysis and the external characters are completely at variance, in that case he is induced to hesitate. He has concluded, and I cannot avoid embracing the same opinion, that identity of composition will generally be attended by identity of characters. When chemical analysis gives us identity in the one, while there exists diversity in the other, it is reasonable to suppose that from the imperfect state of chemistry some important ingredient may have been overlooked by the analyst, which occasions the difference so conspicuous in the characters. What two substances in nature possess properties more different from each other than *charcoal* and the *diamond?* Yet as far as chemical experiments have gone, the composition of both is absolutely the same. It is not possible to avoid suspecting that some essential difference exists between the composition of these bodies, though the art of analysis is not yet far enough advanced to enable us to find it out. Fontana made a great many experiments on the *poison of the viper*, and, as far as chemical properties went, he found it the same as *gum arabic*.

Now, if we consider these two substances for a moment, we cannot but be struck with the great difference of their effects. Gum arabic is an innocent and even nourishing article of food; but the poison of the viper is injurious, and even destructive to life. That such opposite properties should exist in substances absolutely the same we cannot bring ourselves to believe; and cannot avoid concluding that a difference in their composition actually exists, though the imperfect state of chemistry did not permit Fontana to discover it. Now I do not perceive any impropriety in the hesitation of Werner in such cases; it is even praiseworthy, and necessary to prevent mineralogical arrangements from running into confusion.

2. The first part of Haüy's definition being thus insufficient to enable him to arrange minerals into species, it is obvious that he must be guided almost entirely by the second criterion; namely, identity of the integrant molecules. This accordingly is the criterion by which he is really guided, as far as stony minerals are concerned. All those stones that have the same primitive form of crystals he considers as belonging to the same species. Now granting that all minerals are found in a crystallized form, and granting it to be an easy matter to determine the primitive crystalline form with mathematical accuracy, still I affirm that the shape of the crystal has been pitched on to determine the species without sufficient consideration. The atoms of which these primitive crystals are composed have doubtless all of them a determinate form; they may, therefore, (unless we suppose them all spheres) unite by different faces, and of course form crystals of different shapes, not reduceable from each other by any conceivable law: so that it is at least possible that the two parts of Haüy's definition may become inconsistent with each other. Identity of chemical composition may exist along with a diversity in the shape of the integrant molecules. Accordingly two instances of this kind have already occurred. *Calcareous spar* and *arragonite*, and *ruthile* and *octahedrite*, as far as chemical analysis goes, are absolutely identical; yet they differ in the form of their integrant molecules. Mr. Chevenix acknowledges the existence of the first of these exceptions, but he was not aware of the second. He even plumes himself upon the importance of a theory against which only one valid exception could be stated, not considering that one valid exception overturns a theory just as effectually as a thousand.

But Haüy's rule for forming species is attended with other inconveniences of a very important kind. It frequently unites together minerals which possess very different and distinct characters; nor is it impossible that it may separate minerals whose other characters very nearly coincide. What minerals, for example, can be better distinguished from each other than *tremolite, actinolite,* and *hornblende?* Yet the primitive forms of their crystals have been all found to coincide, and they have been recently united by Haüy under the same species. If Haüy go on for any length of time as he has begun, we may predict that all stones will be ultimately reduced to a very small number of species indeed. But such a reduction, though it may coincide well enough with the views of the crystallographer, will make the system quite useless to the mineralogist; because various minerals will be confounded together under the same name, which it is essential for him to distinguish. We request any mineralogist of competent skill to look into the species called by Haüy *quartz,* and see what a number of important minerals are huddled together as a kind of appendage to it. Chalcedony, flint, cats' eye, carnelian, opal, semiopal, pitchstone, menilite, jasper, &c. &c. are all classed together as varieties of quartz. These substances are all as distinct from quartz, and may be as easily distinguished from it, as any species in the whole system of Haüy.

There is one principle, which Mr. Chenevix has turned into ridicule, that it may be proper to notice here, because it is of considerable importance in the Wernerian system, and has induced Werner to arrange the species in a particular order, and sometimes even to alter their position, when the discovery of new specimens render their connection with each other more obvious than it was before. I allude to the doctrine of *transitions.* According to Werner, when species approach each other in their properties, specimens may be found intermediate between them; so as to possess, in a certain degree, the properties of both; and these specimens may be so arranged that they shall decline gradually from the properties of the one species, and approach those of the other. Every mineralogist must have observed such intermediate specimens between *actinolite* and *hornblende,* for example, and between *mica* and *talc.* In these cases the one species is said to *pass* into the other; or there is said to exist a *transition* between the one species and the other. Now where is the absurdity of such a supposition? If species be merely artificial and conventional associations, surely such transitions are natural, and must be expected. Indeed Mr. Chenevix is forced to admit the existence of such intermediate specimens; but he would discard them as monstrosities from the attention of the mineralogist. To ridicule them is the same thing as to maintain that all mineral species have been really formed by nature, and are just as definite as the species in zoology and botany. Yet even in zoology and botany intermediate individuals are known to exist between kindred species, and they have even received a peculiar name from the cultivators of these branches of knowledge. Haüy himself admits the existence of *subspecies*: and he allows that one subspecies gradually passes into another. Of course intermediate specimens or transitions really exist with respect to them. And if so, why not also between kindred species? The example brought by Mr. Chenevix to show the absurdity of transitions is ill chosen, and proves nothing more than that he has not taken the trouble to make himself properly acquainted with the subject. There can be no transition from *sapphire* to the *alumina of Halle*, because the two minerals do not belong to kindred species, and have few or no similar properties.

I have had an opportunity, during the course of my life, of conversing a good deal with gentlemen educated both in the school of Werner and of Haüy; and I must acknowledge that, as far as knowledge of minerals went, I never found any comparison between them. The pupils of the school of Freyberg were beyond all comparison more skilful in ascertaining minerals. This superiority, indeed, is generally known, and will not, I presume, be denied by Haüy himself. If, then, utility is the object which we have in view; if it be our wish to learn the art of knowing minerals when we see them, and of readily distinguishing them from each other, we cannot hesitate, one would think, in preferring that system which gives us this desirable knowledge with the greatest facility, and in the greatest degree.

II. I have dwelt at some length upon the first set of Mr. Chenevix's animadversions, because I consider most of the species into which stony bodies have been divided as arbitrary and conventional, and of course am of opinion that the Wernerian mode of proceeding is the only true and legitimate method of becoming acquainted with the mineral kingdom. The Haüyan mode may answer tolerably well, or excellently if you will, for a few crystallized specimens; but it excludes the great body of minerals entirely, and is therefore unfit for constituting the basis of a mineral arrangement.

It will not be necessary to dwell so long upon the remarks made by Mr. Chenevix on the external characters of Werner. Most of his statements are inaccurate; and his animadversions are, in general, so trifling or absurd, that I can hardly prevail upon myself to believe that a man of Mr. Chenevix's talents and information could put them seriously. I am therefore somewhat at a loss to determine whether, in this part of his paper, he is in jest or earnest. Thus much is certain, that they can produce no injurious effect upon the Wernerian language, or the reputation of its author. For my own part I differ totally from Mr. Chenevix in opinion, and consider the nomenclature of Werner as by far the best that has been contrived for any science, not excepting the botanical nomenclature of Linnæus, or the chemical nomenclature of the French chemists. Werner has displayed a degree of acuteness and exactness, and a talent for observation, which do him infinite honour. But let us notice Mr. Chevenix's principal objections in their order.

1. These characters, it seems, are *too easy* for the philosopher, and may be acquired with *too much facility* for the man of science. They do well enough to discriminate minerals from one another, and are very fit for the miner who has no other object; but they are below the dignity of the philosopher.—This is the first time, I will venture to say, that the facility of a method was thrown against it as a reproach. I admit the objection of Mr. Chenevix in all its force; the Wernerian method is much easier than the Haüyan; and have no doubt that this is one reason why it has been so generally preferred.

2. Mr. Chenevix reproaches Werner for discarding the use of instruments, and for affirming that the senses alone are quite sufficient to enable us to distinguish minerals from each other with the utmost certainty. The truth of the assertion Mr. Chenevix fully admits; but he thinks it quite unphilosophical to confide in the senses, and requires the mineralogist to call in the aids furnished by natural philosophy and chemistry, and to avail himself of the numerous instruments which are employed in these sciences.—I plead guilty to this accusation in its whole force; and have little to offer in vindication of the senses. Thus much, however, I will say. The *sole* object of mineralogy is to enable us to discriminate minerals. Now if one man offers to teach us a method of doing so by the assistance of our senses alone, while another insists upon our calling in the assistance of chemistry and mechanical philosophy, and upon our providing an expensive set of philosophical instruments, I for my part will embrace the first offer, and leave Mr. Chenevix and the philosophers to accept the second.

3. The account of the Wernerian use of colour given by Mr. Chenevix is quite inaccurate, as he may learn from the observations which have been made on the subject in a preceding part of this paper.

4. The account which he gives of Werner's character, *weight* or *specific gravity*, is also inaccurate; as he may see by consulting the treatise on the external characters published by Weaver, or by Professor Jameson.

5. But the most violent attack is made upon the Wernerian mode of describing crystals.

For my own part, I have no hesitation in saying, that I consider the mode of describing crystals adopted by Haüy as greatly superior, in every respect, to the mode adopted by Werner. Haüy has confined himself to the examination of this single character, and he has brought our knowledge of it to a singular and unlooked-for perfection. His mathematical theory of crystallization, and the system which he has built upon it, must always be contemplated with pleasure as one of the most beautiful and useful branches of mineralogical knowledge. The address with which he has employed this character in forming species has often excited my admiration. He has sometimes corrected glaring faults in the Wernerian arrangement, and some of his new groupes of species (as those into which he has divided the *zeolites*) I think infinitely preferable to the old Wernerian arrangement.

But while we give the preference to Haüy, let us not deprive Werner of the merit which he really possesses as a describer of crystals. His sole object was to describe the different shapes of crystals, however irregular, with distinctness, and in as few words as possible. Now I appeal to Mr. Chenevix himself whether this object has not been accomplished. The Wernerian mode of description possesses one advantage over that of Haüy; it may be learned in a few hours by one not even acquainted with mathematics; whereas every mineralogist, according to the method of Haüy, must in the first place be a mathematician. Mr. Chenevix blames the Wernerian terms, because they are not rigidly mathematical. I will not pretend to vindicate them in that point of view. Werner was contriving, not a mathematical, but a mineralogical nomenclature; and, of course, did not consider himself as bound down by mathematical principles. But the Wernerian terms are precise, which was all that was requisite. To give the reader an idea of the indecent strain of writing in which Mr. Chenevix indulges himself, throughout the greatest part of his paper, I shall quote a passage from his animadversions on the crystals of Werner, and I shall quote it in the language in which it was originally written, that I may not be accused of treating him unjustly.

" L'estimation des angles est donnée avec une precision digne de celle qui characterise l'evaluation de la pesenteur specifique. Un angle est tres obtus quand il est plus grand que 120°; obtus, s'il a plus de 100° a 120°; un peu obtus depuis 90° jusqu'a 100°; droit s'il excede 90°; tres aigu entre 45° et 90°; aigu, quand il a 45°; tres aigu, quand il a moins de 45° (Brochant, vol. i. p. 97). Ainsi nous apprenons que l'angle droit est celui qui a plus de 90°. J'ai entendu dire a M. Werner, et j'ai ecrit a ses leçons, sous sa dictée, qu'une difference de 10°, ne l'empechoit pas de considerer un angle comme droit; ainsi nous ne serons pas étonnés tantôt de voir que la zeolithe cubique porte ce nom, puisque le grand angle de ses faces ne differe de l'angle droit que de 3° 30'."

In this passage Mr. Chenevix accuses Werner of not knowing that a right angle is an angle of 90°; an accusation which he could not seriously believe. Who, that has received the least smattering of education, is ignorant of a fact known to almost all the world. I have two observations to make here. This supposed error is quoted from the mineralogy of Brochant, a book published before the author had ever heard a single lecture from Werner. Now what right had Mr. Chenevix to ascribe to Werner an error, however glaring or absurd, which occurred in a book not written by Werner himself, but by a man with whom Werner had no connection whatever. Whoever will take the trouble to turn to the passage of Brochant quoted by Chenevix will find that the phrase *plus que* 90° is an error of the press. This has been already pointed out by D'Aubuisson, and must have been obvious to Chenevix. A man must be greatly at a loss for objections when he is obliged to have recourse to the pitiful shift of an error of the press.

I shall pass by the remaining remarks of Mr. Chenevix on the language of Werner relating to crystals. They are all of a similar cast, and none of them of more importance than those which I have noticed.

6. As to the cleavage, Mr. Chenevix himself must admit that Werner has described it with precision, as far as his observations went. Why blame him for not going farther? When he says that *mica* has *one cleavage*, he has no reference whatever to the number of cleavages necessary to inclose a space. He means merely that he has observed only *one cleavage* in mica. Now this is a remarkable circumstance, and serves to facilitate the discovery of the mineral. Mr. Chenevix, I presume, will not blame Haüy for describing only three cleavages in calcareous spar, which were all that he perceived; though some subsequent observers have since described a great many more.

7. As to the violent attack upon Werner because he so frequently changes the position of his species, it requires no answer. Who made greater, or more violent, or more frequent changes in the position of species than Linnæus? Haüy's species are all unconnected, and constitute in fact nothing better than a catalogue. Any position whatever would be equally proper for each. He has no occasion to change the place of his species, yet his system is not more permanent than the Wernerian, and undergoes as numerous and as violent changes, not merely in the distribution, but in the destruction and resurrection of species. The Wernerian species are connected with each other by transitions. Must not the discovery of new specimens and new affinities enlarge the knowledge of Werner, and oblige him to alter and improve the position of certain species? The science is yet far from perfection; and till it reaches that height changes and improvements must be continually taking place.

8. The attack upon the Wernerian genera of stones proceeds from ignorance or inadvertence. Werner does not maintain that

there is a peculiar earth which stamps its character on all the individuals of a genus. He has no *siliceous genus* at all, for example. He has a *flint* genus, all the individuals of which are distinguished by a flinty hardness, and by a certain approach to the properties of *flint*. The same remark applies to all the other genera. They are named from the most characteristic mineral of the genus, to which all the rest are in some measure referred.

9. Mr. Chenevix has written a panegyric upon the nomenclature of Haüy, and I shall not inquire into the legitimacy of his praises. The conduct of Haüy, in this respect, has always appeared to me very culpable. He has discarded almost all the mineralogical terms which were in common use before his time, and has introduced some hundred new words, all of his own invention. Thus he has added a multiplicity of synonyms to a science already superabounding with them, and in danger of total confusion. What superiority *amphibole* has over *hornblende*, or *pyroxene* over *augite*, I confess, for my part, I never could discover.

III. I ought now to take notice of the attack which Mr. Chenevix has made upon German literature; and his comparison of the state of philosophy, in Britain, France, and Germany. But I have already extended this article to such a length that I have left myself room to touch only slightly on the subject. I am far from attempting to defend the German literary men in every particular. That many absurd books make their appearance in Germany, as well as in other countries, is unquestionable; and that many of their literary men have attempted to distinguish themselves by whimsical or absurd opinions, is too true. I approve neither of their metaphysics, their plays, nor their novels; and I dislike the clumsy and tedious details into which their men of science occasionally enter. But to pretend that they are all destitute of genius, or that they have not contributed essentially to the progress of science, are positions that will not bear examination. Who has contributed more essentially to the improvement of mathematics than Leibnitz, the Bernoullis, and Euler? * and at present do not they possess Gauss, one of the most eminent mathematicians of modern times? Who contributed more effectually to the progress of astronomy than Kepler, whose three laws enabled Newton to develope the theory of gravitation? Who stands higher as a physiologist than Haller? Or who did more to improve chemistry than Stahl, Margraaff, Scheele, and Klaproth? In zoology and botany they have done much, in mineralogy every thing. It is not true that no German has added any thing to the philosophy of chemistry during these

* I include under Germany that part of Swisserland where the German language is spoken.

35 years; unless it be affirmed that the discoveries of Scheele and of Klaproth, and the general chemical law developed by Richter, contributed nothing to the improvement of the science. Such national reproaches as those which Mr. Chenevix casts upon the Germans may gratify spleen, and furnish food for malignity; they may display a happy turn for sarcasm and repartee; but they are not only unjust, but in every respect beneath the dignity of a philosopher, and even inconsistent with common candour and honesty.

Article II.

Account of a Cast-Iron Boiler for evaporating saline Leys. By Mr. William Ramsay.

(To Dr. Thomson.)

SIR, Glasgow, Jan. 16, 1813.

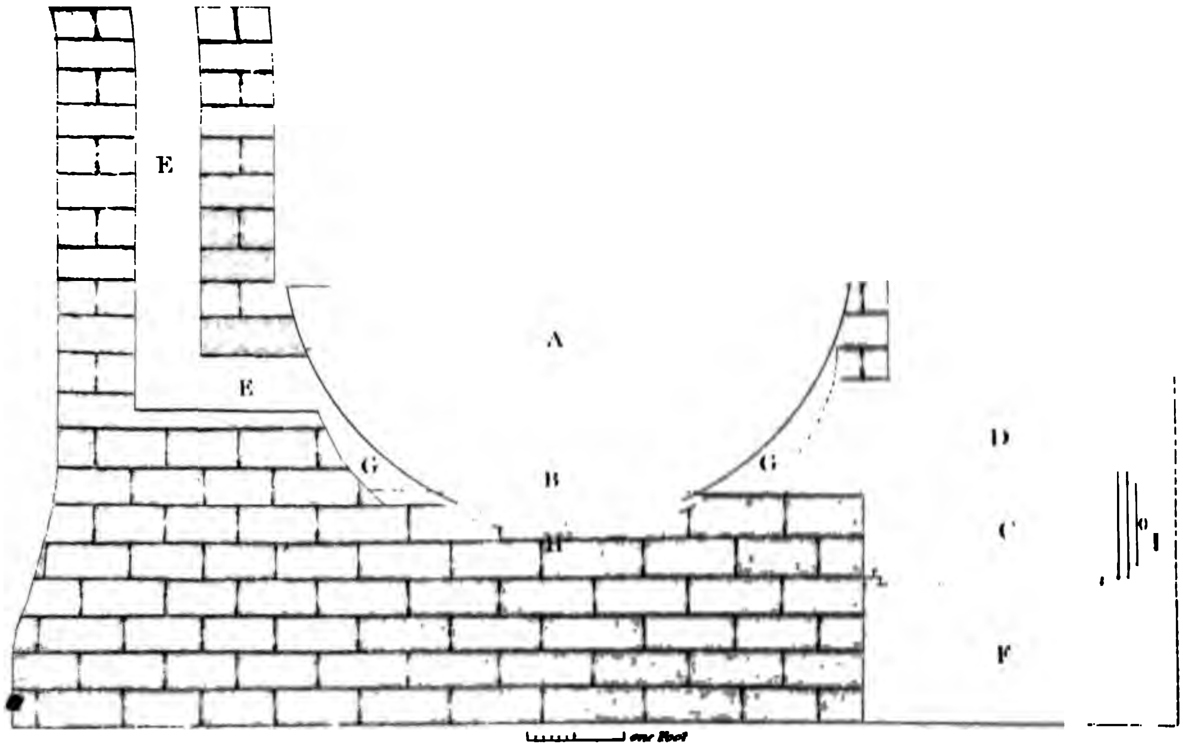
It is a fact well known to those who are interested in chemical works, that boilers of cast-iron cannot be employed with safety either in lixiviating ponderous substances, or in concentrating the solution of any salt which crystallizes at the surface of the liquid by evaporation; because in the former case the mass of the materials resting on the bottom of the vessel; and in the latter, the crystallized salt falling down, is apt to fix on the bottom of the boiler, and ultimately to rend it.

Although boilers made of malleable iron are not subject to the same inconvenience from these causes, yet in a number of cases they cannot be employed with safety: in the solution of a salt, for instance, which contains the smallest predominance of any of the mineral acids, these acting on the joints and rivets, in a short time corrode, and render them unserviceable, which frequently causes not only loss but disappointment.

From these causes I have lost several cast-iron boilers, when the workman did not pay proper attention to the agitating of the salt which was dissolving, or in not removing that which had fallen down by the concentrating of the liquid.

To avoid this expense and inconvenience, I have adopted a method for building up cast-iron boilers, which is particularly calculated for insuring their safety in evaporating any dense liquid; and having now had the experience of its safety for more than two years, I can with greater propriety recommend it to those placed in similar circumstances.

The boilers which I have found most advantageous to use for the evaporation of dense liquids, where the salt crystallizes at the surface by evaporation (such as muriate of soda or sulphate of potash), are those commonly called sugar-pans, which con-



Mr. Ramsay's Cast Iron Boiler for evaporating Soap Leys.

tain from 100 to 300 gallons English wine measure. The form at bottom is nearly a semicircle: they are used in the West India islands for evaporating the solution of sugar, and from long experience are found well adapted for this purpose.

The boiler AB, Plate IV., is a representation of one of these vessels capable of containing 300 gallons; the depth is 2 feet 7 inches, and the width at top 6 feet 2 inches: the bottom of the vessel is set in solid brick-work bedded with fire-clay, as represented in the drawing, to the depth of the dotted line B. The space between A and B is the vacancy where the flue encircles the boiler to heat it, which communicates with the vent E E for the escape of the smoke. The boiler is kept constantly full of the solution which is evaporating above the dotted line A, so as it may not be in danger, when heated, from the cold solution which it may be necessary to add to it.

After the saline solution is so far concentrated that the salt begins to form on the surface, from the peculiar manner in which the boiler is built up, it is evident that the boil must proceed from the circumference to the centre; and the salt, from its density falling down as it is formed, is deposited under the dotted line B in a loose state; and when a sufficient quantity is thrown down it is drawn out by the workman with iron ladles formed on purpose.

From experience we know that salts with a proportion of an earthy basis (such as sulphate of lime) when evaporated in boilers built up in the common mode, with the fire-place directly under the bottom, are deposited, and encrust the bottom so much as to be with difficulty detached from them; and when these deposits increase to any degree of thickness, from the vibration of the boiler by the increased temperature, it is frequently rent when least expected.

On the contrary, when boilers are built up in the manner described above, no accident has ever occurred of this kind with me for more than two years; and I have during that period built up boilers which were formerly so much rent at the bottom as to be no longer useful, but when placed on a bed of fire-clay, supported by brick-work to the depth of the extent of the rent, were rendered completely serviceable.

As to the expense of fuel—until the vessel is brought to the boiling temperature, a strong heat is necessary; but for the continuance of the boil and evaporation, a fire of coal dross is sufficient, when proper attention is given by the man who has the charge of these boilers.

I am, Sir,

Your most humble servant,

Wm. Ramsay.

DESCRIPTION OF PLATE IV.

A B, the boiler; C, the furnace; F, the ash-pit; E E, the vent for the escape of smoke. The bottom of the boiler is set solid in a stiff puddle of fire-clay, and supported by a course or two of brick-work on the ground to the depth of the dotted line B. The flue which surrounds the boiler is built no lower than B, nor higher than A, where it is closed in: the reason of this is evident, because if the flue was lower than B, the deposited salt would not remain in a state of rest; and if higher than A, the boiler might be endangered by the adding of fresh cold liquid for further evaporation.

The fire-place, C, is arched over with fire-brick above the dotted line; hence the fire is allowed its full force on the boiler which it surrounds, until it makes its escape through the vent E E. I, is the door of the furnace with the bars of the fire-place between C and F.

Note by the Editor.—The probable reason why the method recommended in this communication answers so well, is that the temperature of that part of the vessel where the salts lodge, never can rise so high as to produce steam. I conceive it to be steam formed between the salts lying at the bottom and the vessel, that occasions the tremulous motion, which cracks the cast-iron. Mr. Ramsay, by obviating this, has devised a construction which must be of singular utility to the manufacturers of salts.

Article III.

Sketch of the present State of Agriculture in Berwickshire. By the Rev. James Thomson, Minister of Eccles, in that County.

Berwickshire is bounded on the north by East Lothian; on the east, by the German or British sea; on the south, by the river Tweed and Roxburghshire; and on the west, by Roxburghshire and Mid Lothian. According to a late survey, its greatest length is 31¾ miles; its greatest breadth, 19½ miles; its mean length, 26¼ miles; and its mean breadth, 17 miles. According to the estimate of the same surveyor, Mr. Blackadder, it contains 285,440 English acres.

Berwickshire may be divided into two great districts: the Merse and Lammermuir; and two smaller, Lauderdale, and the arable land on the coast. The Merse comprehends the greater part of the low lands: Lammermuir is a ridge of hills on the north side of the Merse. It is to the Merse, and the two smaller districts, that we propose to confine ourselves; in which agriculture has made such wonderful progress during the last half century. It is not, however, to detail the practice of every farmer, but merely to give a concise view of the practice of those who are considered as the most judicious agriculturists.

In order to enable the reader to compare the climate of Berwickshire with counties situated far to the north or south, some account would be proper of the state of the barometer and thermometer; of the winds which prevail; and of the quantity of rain which falls during the year. But it is not usual with farmers to keep meteorological tables. One important observation, however, ought not to be omitted. It appears from records, and indeed from the recollection of old persons still alive, that intermitting fevers were formerly common every season. Such a disease at present is scarcely known. This salutary change is justly ascribed to draining.

Soil.—The soil of the lower parts of Berwickshire is very properly divided by farmers into two sorts, *close-bottomed* and *open-bottomed*. The close-bottomed soil is generally clay, and is so denominated because the under-soil is so stiff and adhesive that rain cannot penetrate. Consequently, the rain water, unless removed by drains, floods the surface of flat or hollow ground. The open-bottomed land is generally loam, and is so called because the under-soil acts as a sieve, and thus carries off the superfluous moisture. Of these two species of soils, the clay and the loam, there are considerable varieties. There is clay; clay and loam mixed in different proportions; and loam blended with more or less sand or gravel.

Object of Farmers.—The object of the Berwickshire farmer is to raise as much grain as possible, and of that kind which is most profitable and suited to his soil. But to produce good crops of corn, manure is necessary; and the best and cheapest is produced by cattle. For this purpose, he keeps as many cattle as he can feed plentifully. Again, experience declares that by extending his views to cattle, as well as to corn, he can calculate his profits with much more certainty; for it often happens that when corn is cheap cattle are dear, and when cattle are cheap corn is dear. He can increase either the one or the other, as circumstances require. Accordingly, every judicious farmer preserves a proper equilibrium between his corn and his cattle.

Size of Inclosures.—All farms in the Merse are inclosed, and subdivided into fields, which generally bear a certain proportion to the size of the farm. Some prefer large inclosures, because they thus preserve for corn what would otherwise, in their opinions, be occupied with unnecessary fences. Some again, who deal extensively in sheep, prefer small inclosures. The size of inclosures may be stated at from 5 English acres to 50, and even to 100 acres, though of so large a size there are not numerous examples.

Size of Farms.—Respecting the size of farms two sorts of average may be taken: we may either compute the number of acres attached to what is called an *onstead*, that is, a farm-house and offices, or offices only; or we may consider the number of acres farmed by one individual, either of lands contiguous or several miles distant from one another. If we take the average size of farms which have a set of offices attached to them, we might venture to say that the most usual size of such farms is from 200 to 400 acres: if, again, we were to calculate the number of acres cultivated by some individuals in the county, we might, perhaps, compute them at from 1200 to 2000 acres.

Rent.—The rent of land upon leases let within the last seven years amounts to from 30*s.* to nearly 4*l.* each acre English. The rent of land, however, it must be remembered, does not, and ought not, to depend entirely upon the quality of the soil. It is influenced also by other circumstances, such as the distance of the market where the produce is sold, and the distance from lime and coals. There can be no doubt, therefore, but land precisely of equal quality is more valuable, by at least 10*s.* an acre, in the neighbourhood of the sea-port of Berwick, than at the distance of 15 or 17 miles.

Leases.—All farms are let upon leases, and almost universally for 19 or 21 years. Either of these periods enable the tenant to lay out a considerable capital in improvements, with the confidence that he shall be refunded with interest before the expiration of his lease. Judicious landlords impose no restrictions upon good tenants respecting the mode of management, except during the last four or five years of their lease.

Capital required. — It is estimated that every tenant ought to have a capital of 500*l.* for every hundred acres he rents, to enable him to purchase stock, seed corn, and implements of husbandry, in order that he may commence his improvements when he enters on his lease; and to prevent bankruptcy, in case of unfavourable seasons and bad crops. Others think a much greater capital necessary. Were the farmer to lay out no capital, and consequently to make no improvements, in many cases the profits of the farm would not be sufficient to pay the rent.

Fences.—Farms are almost universally inclosed, and subdivided by thorn hedges, and ditches five feet broad at top, and three feet deep. These are sometimes cleaned out every fourth year, or every year that the field to which they belong is fallow. They serve as channels for receiving and carrying off the superfluous moisture.

Draining.—On entering upon a new lease, one of the first improvements to which a judicious farmer directs his attention is draining. This is considered as one of the most important, indeed the foundation of all other improvements; for while a field is soked with water it is of very little use. The drains which are most common are intended for carrying off the water that lodges on the surface. Every hollow in a field, which is a natural receptacle for rain, is carefully drained. Drains are usually made three feet deep, where there is a sufficient declivity to carry off the water. Some are made four, and even five feet deep. Where the ground is very flat, it is sometimes impossible to get them deeper than 2½ feet: but such shallow drains are not effectual, unless filled within a foot of the surface with stones. In all cases stones are preferred for filling drains: but so many have already been made, that field-stones are become scarce. In this case old thorn hedges are cut over by the surface of the ground. The stems of these are cut into lengths of one foot and a half or two feet, if the drain be three feet deep. These lengths are then put into the drain, not quite vertically, but leaning upon one another. Where neither stones nor thorns can be had, young firs, or branches of any kind of trees, are employed. Next, the tops of the thorns are pressed in, and covered with straw, or any substance that will prevent the earth from falling down into the drain. Farmers are exceedingly attentive to the filling of the drains, and generally watch the workmen when employed in this operation. At present the common wages for cutting a drain of three feet deep is from 6*d.* to 8*d.* for each rood of six yards; 2*d.* each rood for cutting the thorns; and 2*d.* each rood for arranging them in the drain, and covering them with earth.

Ridges.—Respecting ridges, queries have been circulated to ascertain what ought to be their length and breadth. As to the length, that must evidently depend upon the size of the field after inclosing; and before inclosing, it will be influenced by a variety of considerations, such as the form of the surface, its situation respecting roads, the convenience of the farmers, &c. and consequently cannot be determined as a general question. Nay, in a fine free loam with a pervious under-soil, ridges are not absolutely necessary. They serve useful purposes, however, even in such a soil, to enable the farmer to subdivide in his mind the field into acres, that he may manure and sow by calculation. But in land which has an impervious under-soil, ridges are necessary; or in other words, open furrows are required for receiving and carrying off the excess of rain as it falls. There must be a descent or declivity towards these furrows; consequently, the ground on each side of the furrow must be raised; that is, there must be ridges. As to what the height of the crown, or highest part of the ridge, ought to be above the furrow, that must be such that the rain may run from every part of the ridge into the furrow: but the declivity must be more or less so, according as the soil is more or less adhesive. The point, then, to be attended to in forming ridges in a clay soil, is to make them of such breadth and height that the whole surface water may easily ooze into the furrows. Consequently, it will be found that ridges of 30 or 40 feet will be too broad, unless raised in the middle to an inconvenient height. The most proper breadth in a clay soil is by many judicious farmers supposed to be from 12 to 18 feet, the surface forming nearly the segment of a circle. When the under-soil is pervious to water, the ridges may be flat, and of any breadth; or, as has been already said, ridges are not indispensably necessary. These rules, we believe, are attentively observed by some judicious farmers, and overlooked by others of equal reputation. We may farther observe, that these rules respecting raised and rounded ridges are equally applicable to all impervious soils when the surface is flat, and whatever crops are raised, whether corn, or turnips, or potatoes.

General Rules observed in managing the Soil.—The proprietor of land may be considered as the trader who furnishes the farmer with the raw material. The farmer may be considered as the manufacturer whose object is to alter and improve the soil or raw material, so as to produce the greatest profits to himself. His aim, therefore, is to raise the most lucrative crops, in as great abundance as possible, and at the least expense. He studies with care all the means which experience points out as most conducive to this end. 1. The soil must be drained if wet. 2. As weeds would rob his crops of their nourishment, he must clear the soil as much as possible of weeds. 3. The soil must be plentifully supplied with food for the crops he wishes to raise. 4. The soil must be pulverized, that the minute parts of the food may attach themselves to the small earthy particles, and thus be more easily communicated to the fine vessels of the root, and stems and leaves of the plants: also that the soil may be more accessible to heat and rain. 5. But as it is well known from experience that if a field were to be sown every year with the same kind of seed, the nourishment would soon be exhausted, it is necessary to discover what is the best rotation suited to the soil for procuring lucrative crops by the least exhaustion of nourishment. All these things have been attended to in Berwickshire.

First, then, the land is drained; next, it is cleared of weeds, either by fallow, or by a drilled crop of turnips. If the prevailing weeds be annuals, turnips may be sufficient. But the most prevailing weed in clay soils of Berwickshire is couch grass, which can be banished only by fallow. Fallow, then, is necessary on clay soils, at least when infested with couch grass. But fallow is attended with other advantages besides diminishing or removing weeds. In particular, it pulverizes the soil, and thereby fits it to incorporate in minute particles with the minute particles of whatever is mixed with it for enriching it, whether lime, compost, or stable manure. In the opinion of some, too, the soil is meliorated by the successive exposure of the different parts of it to the sun and air: and therefore they say that a proper interval ought to be admitted between each ploughing, that each new surface may derive benefit from the sun and air.

(To be concluded in our next.)

ARTICLE IV.

Meteorological Tables for the Year 1812. Communicated by Dr. Clarke.

(To Dr. Thomson.)

DEAR SIR,

IN the second number of your *Annals of Philosophy* you have inserted a very valuable paper on the temperature of Stockholm, which shows an ardent desire to advance the science of meteorology. I have sent you the following tables and observations, which may prove interesting to some of your readers. It will be necessary to introduce the table for Sidmouth by a short topography of the place.

Sidmouth is a market town in the county of Devon, on the sea coast, 159 miles from London, 9 from Honiton, and 15 from Exeter. It is situated at the mouth of the little river Sed, protected by hills on the north, east, and west; whilst on the south the sea forms a small bay, which is bounded by Salcombe Hill on the east, and Peak Hill on the west. This bay lies nearly in the middle of that larger bay which is bounded on the east by the Isle of Portland, and on the west by the Start Point. Sidmouth has but one parish; and the population, by the last census, was estimated at 1800 persons. It has deservedly been long the resort of invalids; and many extraordinary recoveries attest the benefit of its equable climate, and the softness of the atmosphere, *very rarely indeed* affected by fog.

Annual Meteorological Table for Sidmouth, Devon. 50° 41′ N. lat. 3° 13′ W. long. By Dr. Clarke.

1812.	Thermometer.—North Exposure.										Barometer.								Weather. Days.			Winds.			
Months.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the day.	Mean for the night.	General Mean.	Greatest variation in 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean of the month.	Greatest range in 24 hours.	Fine.	Cloudy.	Wet.	N and N E.	E and S E.	S and S W.	W and N W.
Jan.	30	49°	S W	23	20°	N E	42°	31°	37°	10°	18	30·39	N	29	29·05	S W	29·96	·72	13	9	9	17	—	8	6
Feb.	19	53	W	6	28	N W	48	36	42	12	19	30·13	W	29	29·09	S	29·64	·70	7	3	19	6	2	14	11
March	7	55	W	17	22	N E	46	34	39	10	9	30·41	N W	20	28·91	S E	29·78	·92	10	5	16	6	11	9	7
April	2	57	W	17	26	N E	52	36	44	8	21	30·31	N W	1	29·57	S W	29·97	·31	22	—	8	10	9	8	10
May	27	66	S W	14	36	S	59	44	51	6	1	30·35	S	29	29·66	S	29·93	·41	14	2	15	4	13	9	8
June	11	71	N	28	37	N E	64	43	54	8	9	30·57	S E	20	29·50	S W	30·03	·48	15	2	13	4	8	12	12
July	8	75	S W	30	36	S E	67	45	56	13	11	30·57	N	28	29·68	N W	30·06	·33	12	3	16	7	5	17	7
Aug.	19	76	S W	7	40	N W	67	46	57	9	13	30·30	S W	19	29·73	S W	30·07	·22	21	2	8	8	4	13	8
Sept.	2	70	S W	18	34	N W	66	40	53	9	12	30·40	N W	28	29·72	S W	30·13	·28	23	2	5	3	6	19	8
Oct.	2	65	S W	28	31	W	58	40	49	14	4	30·09	S W	19	28·81	S W	29·57	·63	11	4	16	3	1	17	13
Nov.	1	57	S	22	24	N	48	36	42	8	22	30 42	N	17	29·08	N	29 99	·42	13	6	11	9	7	8	7
Dec.	1	54	S	13	23	N E	39	31	35	8	28	30·61	N E	17	29·00	N W	30·00	·53	18	6	7	17	5	3	7

ANNUAL RESULTS.

Thermometer.		*Wind.*
Highest....Aug. 19,	76°	S W
Lowest ....Jan. 23,	20	N E
Greatest Variation } Oct. 7—8,	14	S W
Mean for the day }	55	
Mean for the night }	39	
Annual Mean	47	

Wind.	*Times.*	*Wind.*	*Times.*
N	54	S	42
N E	40	S W	95
E	13	W	38
S E	58	NW	66
	165		241
			165
		Total Observations	406

Barometer.		*Wind.*	
Highest	30·61	N E	Dec. 28
Lowest	28·81	S W	Oct. 19

Greatest range, March 3—4, 92°
Annual Mean, 29·93

Weather.	*Days.*
Fine......	179
Cloudy ...	44
Fair......	223
Wet......	143
	366

Annual Meteorological Table for Derby, lat. 52° 58′ *N. long.* 1° 30′ *W. By Mr. Swanwick.*

1812.	Thermometer.—North Exposure.										Barometer.—{170 feet above the level of the sea.								Weather. Days.		Winds.				Rain.
Month.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the day.	Mean for the night.	Mean for the month.	Greatest variation in the 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the month.	Greatest variation in 24 hours.	Fair.	Wet.	N and N E.	E and S E.	S and S W.	W and N W.	In inches and decimals.
Jan.	19	47°	W	16	25°	N W	42°	31°	36°	12°	10	30·18	N	29	29·12	S	29·82	·50	25	6	4	2	8	17	1·15
Feb.	20	52	S	7	30	S W	45	35	40	10	19	30·03	S	28	29·15	S	29·59	·65	20	9	2	6	13	8	1·74
March	30	58	S W	28	23	S E	44	32	38	14	10	30·31	N	28	29·13	S	29·72	·54	16	15	12	8	7	4	4·15
April	15	55	S E	17	26	N	49	33	41	13	21	30·00	N W	3	29·30	W	29·76	·31	20	10	8	9	3	10	·71
May	29	70	S E	3	31	W	59	41	50	18	23	30·11	S	12	29·40	W	29·73	·27	18	13	8	2	13	8	1·86
June	7	72	E	27	34	N W	60	42	51	14	8	30·31	N E	19	29·23	W	29·83	·39	19	11	4	3	9	14	2·12
July	9	78	S E	4	35	S	60	47	53	14	9	30·35	S E	2	29·36	W	29·98	·35	14	16	6	3	13	9	1·92
Aug.	18	72	S	12	38	E	61	47	54	9	14	30·15	S	1	29·75	N E	29·93	·29	19	12	16	1	9	5	1·61
Sept.	21	64	S W	25	30	S	58	42	50	16	12	30·20	S W	28	29·55	N	29·69	·37	22	8	3	5	14	8	1·06
Oct.	4	59	S	27	27	N W	49	37	43	15	4	29·80	S	19	28·34	S	29·26	·51	13	18	—	1	19	11	4·16
Nov.	1	49	S	20	23	E	40	32	36	9	22	30·33	N E	14	29·21	N E	29·77	·49	19	11	9	7	8	6	2·36
Dec.	1	44	S E	19	19	N W	34	29	32	9	7	30·48	N E	17	29·05	N E	29·86	·39	21	10	13	9	3	6	·95

ANNUAL RESULTS.

Thermometer.		*Wind.*
Highest.. ...July 9,	78°	S E
Lowest ......Dec. 9,	19	N W
Greatest Variation } May	18	
Mean for the day	50	
Mean for the night	37	
Annual Mean	43	

Barometer.		*Wind.*
Highest....Dec. 7,	30·48	N E
Lowest....Oct. 19,	28·34	S
Greatest range, Feb.	·65	
Annual Mean........	29·74	

Wind.	*Times.*
N and N E	85
E and S E	56
S and S W	119
W and N W	106
Observations	366

Weather.	*Days.*
Fair	227
Wet........	139
	366

Quantity of rain for the year } 23·79 *Inches.*

General Observations.—The lowest temperature in both Tables is ascertained by a register thermometer on Six's construction, which presents the greatest degree of cold since the last observation, and always refers to a period of 24 hours, although the greatest cold is generally admitted to be about an hour before sun-rise. From an accurate account lately kept of the temperature at 11 at night, it has not been possible hitherto to obtain any rule for comparing these observations, the thermometer falling so irregularly in the course of the night, and in three instances last month (January) as much as 12°. It is highly necessary this should be perfectly understood before any conclusions are drawn from a comparison of these tables. The following tables of temperature, found from observations made at different periods, may nevertheless be found interesting.

1812.		London. Noon, and 11 P.M.	4 miles N.N.W. of St. Paul's. Time not noticed.	Plaistow. Time not noticed.	Carlisle. Time not noticed.	Bristol. Time not noticed.	Derby, by a register therm.	Sidmouth, by a register therm.
Jan.	Highest	—	45°	50°	50°	48°	47°	49°
	Lowest	—	22	26	10	20	25	20
Feb.	Highest	56°	50	54	52	57	52	53
	Lowest	32	26	26	32	27	30	28
March	Highest	57	53	59	53	54	58	55
	Lowest	29	26	24	23	25	23	22
April	Highest	56	57	58	51	56	55	57
	Lowest	28	24	25	30	35	26	26
May	Highest	71	71	76	72	74	70	66
	Lowest	42	52	32	35	44	31	36
June	Highest	74	—	75	76	73	72	71
	Lowest	48	—	29	46	45	34	37
July	Highest	76	—	75	68	75	78	75
	Lowest	49	—	41	47	54	35	36
Aug.	Highest	76	—	78	68	77	72	76
	Lowest	51	—	43	47	53	38	40
Sept.	Highest	70	—	73	64	71	64	70
	Lowest	42	—	34	38	41	30	34
Oct.	Highest	66	65	69	60	65	59	65
	Lowest	34	40	34	33	31	27	31
Nov.	Highest	54	54	55	53	52	49	57
	Lowest	26	26	24	20	17	23	24
Dec.	Highest	53	—	52	48	52	44	54
	Lowest	23	—	18	15	14	19	23

NOTTINGHAM.

1807, Highest temperature, 80°, July
1808, Ditto 89°, July—Lowest temperature, 17°, Jan.
1809, Ditto 78°, July Ditto 17°, Jan.
1810, Ditto 82°, Sept. Ditto 14°, Feb.
1811, Ditto 13°, Jan.

Mean for the 4 years, 48·75°.

SIDMOUTH.

1811,.................................Lowest temperature, 22°, Dec.
1812, Highest temperature, 76°, Aug. Ditto 26°, Jan.

I do not think the tables will require any further explanation; the plan is very obvious.

I am, dear Sir,
Your obedient servant,
JAMES CLARKE.

Sidmouth,
Feb. 10, 1813.

Article V.

Extent of the Counties of Scotland; ascertained by Mr. Jardine and Sir George Stewart Mackenzie. Communicated by Sir George Mackenzie.

In order to ascertain, with as much precision as possible, the superficies of Scotland, and of the different counties into which it is divided, a copy of Arrowsmith's map was selected, the paper of which was nearly of uniform thickness. A portion of each sheet, equal to 5000 English square miles, measured from the scale of the map, was carefully weighed; the balance used in this, and the subsequent operations, being sensible to the one-hundredth part of a grain, when loaded with 2lbs. in each scale. Each county was then accurately separated by means of a sharp-pointed knife, and its weight compared with that of the portion of the sheet to which it belonged. In those counties which contained a considerable portion of *fresh* water lakes, the lakes also were separated and compared in a similar manner; and from these data the surfaces of the land and water of each county were deduced. The map of Scotland constructed by Arrowsmith is undoubtedly the best that has hitherto been published. It does contain a few errors in the positions of places, and a few in the courses of rivers; but none have yet been discovered that can affect the present calculation in any material degree. Indeed, if Arrowsmith took greater pains to render one part of his map more accurate than another, it was in ascertaining the boundaries of counties; and in doing this, he was liberally assisted by the Parliamentary Commissioners for Highland Roads and Bridges. On the whole, the following Table may be regarded as the nearest approximation to the truth hitherto attempted. The method employed seems to have been first made use of by Dr. Long, of Cambridge, in the year 1742, to ascertain the proportion of the land to the water, on the surface of the earth.

It must be observed, that from a recent comparison, made with great accuracy, of the Scotch standard ell, and the English standard yard, it appears that the Scotch chain should be, at the temperature of 60° of Fahrenheit, 74·1234 English feet, instead of 74·4, the length of the chain in common use. In the calculations by which the Table has been constructed, the correct length was used, which makes a difference of above 11,000 Scotch acres to be added to what the sum would have been, had the common measure been employed.

COUNTIES.		ENGLISH SQUARE MILES.	ENGLISH ACRES.	SCOTISH ACRES.
Aberdeen		1934·56	1238080	981580
Argyll	*Mainland*	2212·34	1415898	1122559
	Islands	785·65	502816	398645
	Water	32·11	20554	16395
Ayr		1042·01	666886	528724
Banff		632·60	404864	320986
Berwick		478·52	306253	242805
Bute		153·98	98547	78131
Caithness	*Land*	737·79	472186	374360
	Water	6·45	4128	3273
Clackmannan		52·55	33632	26664
Cromarty	*Land*	253·83	162451	128795
	Water	8·57	5485	4348
Dumfries		1271·40	813696	645118
Dunbarton	*Land*	246·17	157549	124909
	Water	32·54	20826	16511
Edingburgh		387·49	247994	196635
Elgin		472·02	302093	239507
Fife		521·44	333722	265593
Forfar		977·97	625901	496230
Haddington		290·96	186214	147635
Inverness	*Mainland*	2726·65	1745056	1383524
	Islands	1035·00	662400	525167
	Water	83·79	53626	42496
Kincardine		400·91	256582	203425
Kinross	*Land*	77·07	49325	39106
	Water	6·76	4326	3430
Kirkcudbright		814·51	521286	413289
Lanark		993·61	635910	504166
Linlithgow		134·27	85933	68130
Nairn		196·65	125856	99782
Orkney Islands	*Land*	313·75	200800	159199
	Water	9·15	5856	4643
Shetland Islands		516·62	330637	262137
Peebles		347·10	222144	176121
Perth	*Land*	2830·30	1811392	1436116
	Water	33·58	21491	17039
Renfrew		232·49	148794	117967
Ross	*Mainland*	2033·98	1301747	1032057
	Islands	561·17	359149	284742
	Water	39·42	25229	20002
Roxburgh		725·81	464518	368282
Selkirk		265·91	170182	134925
Stirling		532·33	340691	270108
Sutherland	*Land*	1865·53	1193939	946585
	Water	37·86	24230	19210
Wigton		442·78	283379	224670
Total	*Land*	29497·66	19078502	14966374
	Water	290·23	185751	147347

Note—By the term Water, in the Table, is to be understood only the *fresh* water of considerable lochs or lakes, that of rivers and *salt* water friths not being included.

Article VI.

Exposition of the Facts hitherto collected concerning the Effects of Vaccination, and Examination of the Objections made at different Times against the Practice. Read to the Class of Physical and Mathematical Sciences of the French Institute, by M. M. Berthollet, Percy, and Hallé, August 17, 1812.

(*Continued from* N° II. p. 143.)

THIRD QUESTION.

Is the virus introduced by vaccination of such a nature as to produce immediately, that is, during the developement of the natural effects of vaccination, fatal accidents?

Here a second class of facts make their appearance, which have been brought forward as a proof of the inconvenience and even danger of vaccination. These consist in an enumeration of the fatal accidents which have taken place during the process of vaccination. Let us consider these facts.

The first observation is drawn from the work of Dr. Woodville. The child was at the breast.* On the 9th day after vaccination there broke out from 80 to 100 pocks, accompanied with frequent spasms; and the child died on the 11th day. Now as we have shown that the phenomenon of eruptions has nothing to do with the cow-pox, we ought to infer that fatal accidents following eruptions are not the consequence of vaccination. Hence, from the preceding observation, no consequence unfavourable to vaccination can be drawn.

A second case, worthy of remark, is taken from a report of Mr. Moore, which M. Chappon cites as a proof of the dangers resulting from vaccination.† The 8th or 9th day after vaccination, an infant of three years of age, till that time in good health, was afflicted with a cough. On the 12th day he lost his voice. On the 14th he was afflicted with a sense of stifling, which increased on the 15th day. The respiration became difficult, rapid, noisy, and the noise appeared to proceed from the windpipe. The same day he died. Here it is impossible to avoid perceiving the symptoms of croup; but as this dreadful disease often attacks infants on a sudden, and during a state of perfect health, and advances with terrible rapidity; why should we, when it happens to occur after vaccination, ascribe it to that operation, when it is well known to proceed from causes totally different? This fact, then, ought not to be ranked among those which demonstrate the dangers of vaccination.

Ought we to include among the effects of vaccination the

* Report on the Cow-pox, p. 103. French Trans. † Chappon, p. 104.

convulsions which carried off an infant, the death of whom is ascribed to vaccination by M. Chappon, and those which carried off another on the 6th day of a pernicious fever?* Are fevers and convulsions so rare among children, that when they happen to occur during the developement of the cow-pox, or soon after, we ought, without farther proof, to ascribe them to the influence of vaccination?

We find likewise in the work of M. Chappon the case of a child of M. Goupy, a child covered with a scurvy eruption (*gourme*), which suppurated abundantly.† During the course of the cow-pox the eruption was mixed with a great many pocks. It assumed a character of extreme acridity and putrefaction, occasioned violent pain, and produced convulsions which terminated in death.

Though the consequences drawn from this case appeared very equivocal, we thought ourselves obliged to apply to M. Lafisse, a physician of known merit, under whose eyes the patient was. He gave us the following answer:—

"The child of M. Goupy, about whom you inquire, had been vaccinated without my knowledge. She had her head covered with a scab (*gourme*), and her humours very much vitiated. Her father requested me to visit her on the 9th or 11th day of her disease, which had commenced soon after her vaccination. I found her labouring under a severe typhous fever, under which she sunk two days after, in spite of the blisters and the bark which I instantly prescribed. Besides the cow-pocks at the places where the virus had been introduced, several others had appeared on her head, and on other parts of her body. I believe that the fever had nothing to do with the vaccination; that the vaccination may perhaps have contributed to determine it, but to which she had a strong previous disposition. The only reproach is that the cow-pox was introduced in circumstances so unfavourable. This is all I can say about that accident, of which I was a melancholy witness.

We shall add nothing to these reflections of M. Lafisse, the justice of which appears evident to us. A similar example, but terminating less fatally, has been mentioned to us by a person worthy of confidence. We endeavoured to verify it, and we found that the fact was only attested by false reports, by means of which that person had been deceived.

It is evident, then, that in none of the cases of which we have spoken can the fatal result be ascribed to the influence of the cow-pox virus. We shall now notice the facts furnished by the correspondence of the society of Paris, and those which the authors of the Bibliotheque Britannique have published relative

* Chappon, p. 108, 109. † Ibid. p. 102.

to the deaths which have followed soon after inoculation with cow-pox virus.

The instances of this kind contained in the correspondence of the society of Paris are 11 in number. Four children died of the small-pox coming on the 2d, the 6th, the 8th, and the 9th day after vaccination. This disease had of course been contracted before vaccination, or at least before it could produce its preservative effects. Two other children died of convulsions. One of these was only three months old, and had been subject to convulsions from its birth. The other was tormented with convulsions in consequence of worms, and the disease had existed before vaccination. Five infants newly born, two of whom were afflicted with a syphilitic disease, and three were in a state of marasmus, died soon after vaccination. These are the only examples noticed in the registers of correspondence out of more than two millions and six hundred thousand persons vaccinated, and not one of them can be ascribed to the cow-pox, since independently of it there existed diseases sufficient to occasion death.

The extracts inserted in the Bibliotheque Britannique furnish us, likewise, with examples of death following soon after vaccination. Several, during the time that the small-pox was epidemic, were occasioned by the small-pox itself, which appeared immediately after vaccination. This happened both at Geneva and in other places.* In other cases death was occasioned by peculiar eruptions complicated with the cow-pox, and we have seen what ought to be concluded with regard to such a complication. A fatal accident, which happened at Nottingham in 1801, was occasioned by an universal erysipelas observed upon two infants, and which carried off one.†

If we now compare the number of deaths with the number of vaccinations which terminated fortunately, we shall find that the infant mentioned by Dr. Woodville in 1798 was still the only one in 1799 out of 6,000 vaccinated;‡ and that in 1807, according to the report of the surgeons of London, only three deaths had been observed out of 164,381 vaccinated, which amounts to 1 in 54,793·6.§

From the facts which we have stated, it appears that the fatal accidents observed during inoculation with the cow-pox have taken place in the case of eruptions produced by the small-pox existing at the same time, by convulsions for the most part existing before vaccination, in one case by the croup, by marasmus already far advanced, by the syphilitic virus, and by

* Bibl. Brit. vol. xv. p. 84, 85; xliv. 290, 291.
† Bibl. Brit. vol. xvi. p. 298.
‡ Bibl. Brit. vol. xii. p. 325; xiv. 200.
§ Bibl. Brit. vol. xxxvi. p. 371.

the complication of a scurvy eruption of a bad character joined with a predisposition to a typhous fever. Thus none of the deaths can be ascribed to the nature or character of the cow-pox itself: all are the consequence of other diseases well known, or produced by accidents; the causes of which, independent of vaccination, but coincident with it, may be completely appreciated by what we have said above.

FOURTH QUESTION.

Is the virus introduced by vaccination of such a nature as to produce, even after its operation is happily finished, diseases, more or less severe, and which may even prove fatal?

The solution of this question is difficult, because our investigation is of necessity interrupted by a great number of uncertainties.

It is certainly difficult to establish, that a virus, introduced into the body, and capable of rendering it inaccessible to the small-pox contagion, has not the power of producing any other change which can affect the health. Such a consequence can only be the result of a number of observations, so great that its disproportion with the contrary observations prevents us from ascribing them to any thing else but causes absolutely unconnected with the introduction of the virus.

But the observations in support of a contrary opinion must be equally difficult to obtain. If a disease appears after vaccination, in order to show that it can be ascribed to no other cause, we ought to know what was the state of the subject before vaccination, and whether his constitutional or hereditary dispositions did not prepare him for those maladies which have taken place. We must be able to show that after vaccination he has not been exposed to causes capable of producing these diseases. We ought likewise to inquire whether the source from which the cow-pox matter was derived was infected with any foreign ferment. And finally, as in all ages and all circumstances of life various diseases appear which cannot be assigned to any known cause, those which succeed vaccination ought, in order to be ascribed to it, to show such a character of affinity with each other as to indicate their common origin, and offer in their developement a connection more or less sensible with the primitive effects of vaccination to which they succeed.

It is therefore requisite to admit, in opposition to the advantages ascribed to vaccination, those observations only which are well authenticated, and the details of which are sufficiently complete to enable us to appreciate their value.

Nevertheless, if the number of facts alleged were very considerable, as it would be impossible in such a case to ascribe them to mere accident, they would in a great measure supply the place of exact observations, and would produce a certain degree of probability in their favour.

By attending to all these particulars we shall endeavour to give an answer to the question proposed.

We shall begin with the observations which have been given as proofs that there exist diseases which owe their origin to vaccination.

Among those that have been published, or that have come to our knowledge, there are very few which, considered separately, have the character of exact observations; and not one possesses the conditions necessary to fix the relation of the malady noticed to the previous vaccination.

Out of eleven observations that have been particularly communicated to us, and which, from the precision with which the facts were announced, as well as the nature of the evidence of those who communicated them, seemed to deserve particular attention, we have had it in our power to verify seven. All of these seven were formally and authentically denied by occular witnesses, most assiduous, and consequently best acquainted with the facts, either from situation, or the interest which attached them to the children who were the subject of these observations. We can only suppose that the persons, who communicated to us these observations, who were well-informed persons without any motive to deceive, were led into error by false reports concerning things which they had not been able to see with their own eyes. After this it was natural for us to suspect the authenticity of the other facts which had come to our knowledge by the same means, though we had it not in our power to verify them by actual inquiry.

A fact reported to the medical society of Grenoble has been mentioned, and it is advanced in the work of M. Chappon as a proof of the bad effects of vaccination.* A child after vaccination had the face covered with pimples, which were succeeded by scabs that gave the face a hideous appearance. This was followed by an anasarca, and the case ended fatally. Notwithstanding the want of details in this case, it is easy to perceive in it that eruption so familiar to infants, and known by the vulgar name of *croute laiteuse* (*crusta lactea*). Its appearance after vaccination does not prove that it had any thing in common with it. We frequently see the suppression of such eruptions produce very severe symptoms without the presence or vaccination, commonly either in the head or the organs of respiration.

The little exactness in the other observations which we might

* Chappon, p. 134, 135.

examine, renders it impossible to admit them as proofs in a discussion like the present.

We have met with strangers to the art of medicine, especially parents, who have assured us that their children, after having been carefully and successfully vaccinated, experienced several inconveniences, sometimes eruptions, sometimes a weakness of health to which they had not been subject before vaccination. These symptoms in some cases obliged them to have recourse to blisters and issues in order to remove them. It was impossible for us to make ourselves so well acquainted with the origin of these facts as to be able to judge how far the allegations were well founded; but without rejecting them altogether, we may say that all the children, and even adults, that we have had an opportunity of vaccinating ourselves, or that we have seen vaccinated, never exhibited any such symptom.

There is a circumstance which we observe frequently, and to which we ought to attend particularly, while discussing the present question. We often see an accidental impression, an emotion, a fall, occasion the developement of a disease, to the nature of which that occasional cause is obviously a stranger. The small-pox itself often appears after such accidents, and in other cases they have occasioned violent fevers or other maladies to which a disposition seems to have pre-existed, and only required an occasion to call it into action. Is it not also possible that in circumstances which we can neither determine nor foresee, vaccination may give occasion to the appearance of a malady, without being its cause, and thus bring about what any other commotion would have done, experienced at the same time? In that case there would be nothing in such diseases connected with vaccination, or proceeding from the cow-pox virus.

Since then there is not one of the observations, collected hitherto, which can of itself serve as a proof of the opinion which we are examining, it remains for us to see whether taken collectively their number is such, compared with that of the cases whose history is known, as to give some solidity to the objection.

The collections to which we have had recourse already, in order to give an answer to the other questions, will still furnish us with numerous facts to satisfy this.

The correspondence of Paris, besides the facts which we have noticed above, furnishes the following; erysipelas in the arm in the proportion of one case to 10,000; suppurations continuing in the cow-pock, in the proportion of 1 to 10,000; and these are only local accidents, particular to the parts on which the inoculation was performed. As to general accidents they have only been observed when from particular objects the number of punctures has been very much increased, as when they have amounted to 30, 40, 50, or even to 60. These accidents have been fever and convulsions, which did not in any instance terminate fatally. The cases collected by the Society of Paris are all such as have exhibited the characteristic progress of true cow-pox, an observation of more importance than has always been supposed.*

The facts furnished by the Bibliotheque Britannique give us the following results. We shall notice those only which have been announced with so much precision as to give us an exact idea of the case.

In 1800 M. Odier announced at Geneva that out of 1500 persons vaccinated not one accident had occurred.†

Dr. Anderson writes, in 1804, from Madras, to the Jennerian Society of London, that the number of vaccinations performed by the British and Indian physicians on English, Portuguese, Brahmin, Malabar, Gentoo, Mahometan, Half-cast, Pariah, Maratta, Canadian, and Rajaput subjects, amounted to 145,848; and that in none of these cases had a single accident been observed.‡ This enumeration was made in 1803, and published in 1804 by the Government of Madras.

In 1806 the Jennerian Society of London, in consequence of rumours propagated respecting vaccination, as if it occasioned various dreadful diseases till that time unknown, was induced to make an exact examination. The result of this, comprehended in 22 paragraphs, gives in paragraph 21 the following statement: The disease produced by vaccination is in general slight, and without bad consequences. The cases contrary to this conclusion are in small number, compared with the total number of cases, and may very naturally be ascribed to the constitution, or the peculiar disposition of the individuals who have exhibited the exceptions.§

In 1807 the Society of Surgeons in London published another report, more precise; and in which they show the greatest reserve with respect to the consequences to be drawn from the results obtained. We have already said, in speaking of the eruptions following vaccination, that there were only 66 examples of them among 164,361 persons vaccinated: 24 erysipelatous affections only were observed out of the number 66; and

* Jenner's further observations, Bibl. Brit. vol. xv, p. 269.

† Bibl. Brit. vol. xvi. p. 99.

‡ Bibl. Brit. vol. xvi. p. 99. The details are as follows:—English, 165; Portuguese (Creoles), 1092; Brahmins, 4141; Malabars, 41,806; Gentoos, 40,022; Mahometans, 10,926; Half-casts, 444; Pariahs, 35,985; Marattahs, 440; Canadians, 10,367; Rajaputs, 462. In all, 145,848. This enumeration is not superfluous here, as it presents the effects of vaccination in all possible relations depending upon the difference of man, and the variation of habits and circumstances.

§ Bibl. Brit. vol. xxxii. p. 75, 82.

among these we must reckon the only three deaths which followed vaccination, and which have already been noticed. All this is the result of the answer of 426 correspondents, whose testimony was solicited by a circular letter.*

In another place mention is made of the same erysipelatous cases, probably comprehended under the 24 which have been just mentioned. The disease is ascribed to the too great depth of the incisions, by means of which the cow-pock matter had been pushed too far below the skin, instead of being introduced between it and the epidermis. Other observations may give some probability to this presumption, which we shall not attempt to examine here.†

At Aleppo the English Consul, Mr. Barker, has succeeded in familiarizing the people to vaccination: 600 were vaccinated in 1806, without observing a single disagreeable accident to follow.‡

In 1803 the Spanish Government undertook the noble and generous enterprise of sending out an expedition, which terminated in 1806. The sole object of this expedition was to convey to all their American and Asiatic posssessions the new means of preserving the colonies against the ravages of the small-pox.

A certain number of children was embarked, who were to be vaccinated successively during the voyage. In this manner the cow-pock virus was transported to the Canaries, to Porto Rico, to the Caraccas, to Guatimala, to New Spain, to the Philippine Islands, to Macao, to Canton, to the islands of Visaye, where a hostile nation was so struck with this act of generosity on the part of the Spaniards as immediately to lay down their arms. The colonists of St. Helena, who had hitherto refused the cow-pock matter from their own countrymen, received it from the Spaniards The provinces of Terrafirma, of Carthagena, of Peru, &c. likewise received the cow-pock matter, which was even found indigenous near Puebla-de-los-Angeles, not far from Valladolid, and in the Caraccas. The Viceroy of New Spain has attested that out of 50,000 individuals vaccinated in his government not a single unfavourable accident had come to his knowledge.§

At Echaterinoslaff, the Duke of Richelieu, Governor of the

* Bibl. Brit. vol. xxxvi. p. 371.

† Bibl. Brit. vol. xvi. p. 298. The different effects of superficial insertions, and of insertions in the tissue of the skin itself, are remarkable in the inferior animals, especially in sheep. In them inoculation under the epidermis is found efficacious, and free from accidents; but if it be pushed deeper, it is followed by entiraces, gangrenes, &c. the tissue, the functions, and the properties of the skin differ at these different depths. Annales d'Agriculture, Obs. de M. Picot La Peyrouse, vol. xlvi. p. 281, 286, 293, 294.

‡ Bibl. Brit. vol. xxxii. p. 394, 400.

§ Bibl. Brit. vol. xxxv. p. 239. The report of this expedition was made to the King of Spain by Dr. F. X. Balmis, surgeon to the King, and one of the chiefs of the expedition.

Crimea, assures us that out of 7065 individuals vaccinated in six months, not a single accident intervened, except one, in which the small-pox appeared the day after vaccination.*

Finally, in 1810 M. Curioni, Minister of the Interior at Milan, wrote to M. Sacco that as far as his information went not a single instance had occurred of small.pox appearing upon individuals that had been vaccinated, and no disease whatever had followed the process.†

It appears to us that the small number of unfavourable observations which have been collected, and among which we must not include those not well authenticated, and which depend upon assertions destitute of proof, disappear entirely before such a mass of facts.

FIFTH QUESTION.

Supposing that inoculation for the small-pox has the advantage of sometimes favouring the cure of certain chronical diseases, is this advantage peculiar to it, and ought it to ensure it a preference over vaccination?

This fifth question does not present fewer difficulties than the preceding.

In speaking of the diseases, the origin of which has been referred to vaccination, we might have observed that the same reproach had been thrown against the small-pox, and that not without some reason. Not to mention former authors suspected of partiality, we shall satisfy ourselves with referring to the authors of the Bibliothéque Britannique, who have given some instances.‡ Other facts of an opposite nature have been alleged, showing that inoculation is an epoch of an advantageous change in the constitution, by the cessation of various infirmities, and the confirmation of the health and constitution of the person inoculated.

These advantages have been ascribed either to the perfection of the eruption, and the regularity of the general commotion which accompanies it, or regarded as the effect of the suppurations prolonged in the place where the inoculation was performed; a phenomenon which has been imitated by means of a supplimentary suppuration, induced by blisters when the circumstances of the case seemed to require it. It has been conceived that these evacuations destroyed the causes of the diseases formerly existing, and in the midst of which the small-pox had made its appearance.

Observers will not consider it as a contradiction to say that a

* Bibl. Brit. vol. xliv. p. 286.

† Bibl. Brit. vol. xlv. p. 228; and Trattato della Vaccinatione, p. 210.

‡ Bibl. Brit. vol. ix. p. 295.

commotion excited by the introduction of the matter of small-pox may produce results that seem diametrically opposite to each other. These effects do not appear contradictory, but because they vary according to the disposition and the strength of the subjects who receive the virus, and according as the essential phenomena of the malady, which this virus occasions, take place with more or less violence, regularity, or perfection. The fact exists. The only conclusion, which in our opinion can be drawn, is that these effects depend upon general laws, which it is not our business here to explain, and that they must not be regarded as a specific property, which, if it did exist, could not give birth to consequences so different.

We must, however, acknowledge, that however striking the observations may be, they do not lead to a striking demonstration. Hence, when any person says that inoculation favours the cure of a particular disease, we must restrict the proposition to mean nothing more than a simple expression of the particular fact observed. A person was afflicted with a chronic disease, from the knowledge of the character and progress of which we could not expect a speedy cure. This person was inoculated, and soon after the cure took place in a manner quite unexpected. Such is the fact. To draw as a consequence that the inoculation was the cause of the cure, it would be necessary that analogous instances had either always, or at least very frequently, occurred; otherwise the coincidence may have been entirely accidental.

Examples are given of obstinate, even hereditary ulcers, of cachexy, scurvy, eruptions, &c. cured in consequence of inoculation. The character of those who have attested these facts does not permit us to call them in question. We readily admit them; but to prove that these advantages ought to establish a preference for inoculation with the small-pox matter over vaccination, it would be at least necessary to prove that vaccination has not been followed by equally fortunate consequences; but the very contrary fact results from the observations collected by the Correspondence of Paris, and from several cases announced in the works extracted by the authors of the Bibliotheque Britannique. The variety of facts announced by the Correspondence of Paris is so great that it might even lead to some scepticism. We shall therefore only notice those relations which are given by persons entitled to draw our attention, and those the details of which contain some interesting particulars. Without attempting to draw any consequences from them, we shall simply present a short statement.

Mr. Richard Dunning, of Plymouth, in a work published in London in 1800, entitled *Some Observations an Vaccination, &c.* when speaking of the effects of vaccination on the health, says, that he has generally observed the health improved by vaccination, and he gives two instances: the first a young girl, daughter of a consumptive father, subject to vomiting, and continually labouring under oppression, with a cadaverous aspect spotted with livid blotches. After a fortunate and successful vaccination, she in a few months recovered the best possible state of health. The second example was a child two years of age, naturally delicate, recovering from an inflammation of the breast, but still pale, very feeble, and oppressed. This child, after vaccination, speedily recovered strength, acquired a good habit of body, a free respiration, and an excellent state of health. M. Maunoir, of Geneva, on this occasion adds another instance: a child, whose arm was covered with dartrous eruptions, which inflamed during the influence of the cow-pox inoculation, and assumed the appearance of as many cow pocks. After the vaccination was over this child got quit of the eruption entirely. The same person affirms that he has observed, even after false vaccination, a sensible improvement in the health of delicate infants.*

Similar results have been announced in the Spanish expedition, with an intention to publish them.†

Dr. Sacco, in his treatise *Della Vaccinazione* (Milano, 1809), affirms, that when vaccinating infants affected with palsy in the arms or lower extremities, troubled with chronic diseases of the glands, &c. he made a great number of punctures on purpose, to the amount of 30 or 40: that some of these patients were perfectly cured, and that the health of others was considerably improved.‡

M. Barrey, of Besançon, observes that vaccination had been performed, in 1804, in three villages belonging to his department, on 141 infants under 12 years of age, constituting more than one half of all the children under that age in the place. In 1809 no fewer than 134 of these children enjoyed perfect health, 7 alone having died of different diseases; but of the children that had not been vaccinated no fewer than 46 were dead, though no small-pox had visited the country during the period. If under this last number be only included the children that existed in 1804, and not those born between that period and 1809, we must conclude that vaccination had rendered the children less susceptible of other diseases; but Mr. Barrey's observation is not sufficiently precise to enable us to estimate its importance.§

The facts contained in the Correspondence of Paris present themselves in a much greater number. If we refuse to admit

* Bibl. Brit. vol. xv. p. 383. M. M. La Mark and Gaultier de Claubery have communicated to us observations to which they were witnesses, very similar to those of Mr. Dunning.

† Bibl. Brit. vol. xxxv. p. 245.

‡ Trattato della Vaccinazione, p. 112; and Bibl. Brit. vol. xlv. p. 168.

§ Bibl. Brit. vol. xxxix. p. 95. De la vaccine et de ces effets par C. A. Barrey; Besançon, 1808. See also Bibl. Brit. vol. xxxvi. p. 352.

all these cures to be owing to vaccination, we shall at least allow the coincidence of the cures with vaccination. Even in that case the great number of facts must produce at least a suspicion that vaccination had a useful effect in these cases, and give us a certainty that at least it was not injurious.

The names of the observers, the places where the observations were made, the kind of observations, are marked with precision in the notes which have been put into our hands. A considerable number enter into details, both respecting the phenomena and the methods employed; the number of punctures made in order to induce a more considerable commotion, and to render it more general and more efficacious.*

We ought to remark here more particularly the maladies which affect the organs and functions which belong to the lymphatic system. On that account we shall begin with them. Fourteen observers have given a great number of examples of the crusta lactea disappearing after vaccination; sometimes after a suppuration of the cow-pox continued for 27 days. Seven observers have sent a great number of observations, two of which are accompanied with details, stating the termination of dartrous affections spread over different parts of the body, and especially the arms, after vaccination. In one of these cases the cure was preceded by a violent inflammation round the cow-pox, and by a suppuration kept up for a month. Eighteen observers have given an account of chronic and obstinate opthalmias in scrofulous children cured by vaccination. Eight of these observations are detailed. In several cases the punctures made amounted to 15 or 20. Some were made in the nape of the neck. In most of them the suppurations were long continued; sometimes they were succeeded by blisters: but in every one of the cases the same means had been employed before vaccination without any effect. Twelve observers have given numerous facts relative to the termination of scrofula after vaccination. Eight of these are detailed. In one the scrofula was complicated with opthalmia. Sixteen punctures were made in the limbs. On the seventh day the child opened its eyes, and was capable of bearing the light. The inflammation of the punctures was violent: the inguinal glands subsided, the scrofulous tumors disappeared, and the cure was complete; but it was thought proper to endeavour to render it still more secure by a cautery performed on one of the limbs. In another case the scrofulous tumors were open, they discharged an unhealthy pus, and the flesh was pale and fungous. During the progress of the cow-pox the edges of the ulcers became red, and the flesh firm; the suppuration became less abundant, and less watery; much of the humors was drawn

* See the reports and notes of which we have spoken above.

to the vaccinated arm; the scrofulous tumors healed in the course of a month; the cow-pox continued to suppurate during three months, and then the cure was complete.

Since the introduction of vaccination into the department of Mount Blanc, M. Caron, Physician of Annecy, affirms that the number of scrofulous diseases has sensibly diminished; and M. Bacon, physician at Falaise, that in the hospital for children, formerly filled with scrofulous cases, no such disease is now to be found. Four observers sent various observations, five of which are very detailed, and have for their objects cases of rickets, not indeed cured, but modified in a remarkable manner, and the progress of which was either stopped, or sensibly retarded, by vaccination. The power of walking recovered, strength increased, and the solidity of station re-established, were the most sensible effects that resulted; and in these cases the numerous punctures along the spines were the means by which they flattered themselves with having obtained success. Three observers have spoken of the tinea capitis. One of the observations is detailed, and gives an account of a tinea of a yellow colour, yielding a copious yellow humour, of the consistence of honey. Twelve punctures were made upon the head itself. When the vaccinal crusts fell off, the crusts of the tinea dried up, fell off, and the cure was complete. Five observers furnish numerous facts respecting vaccination performed on patients labouring under nervous disorders. Five of these are detailed. A megrim which continually tortured a young man of 14 years of age, for several years, vanished after the suppuration of the cow-pox. Daily convulsions, during ten months, in a child of 20 months, which had not been alleviated by medicine, became less violent during the progress of vaccination, and afterwards disappeared altogether. Various convulsive diseases, three of which were epileptic, were suspended during the progress of the cow-pox. Afterwards they continued to recur, but at longer intervals. Three of them, one of which was hereditary, ceased altogether. In one that had convulsions every day, the vaccination was performed during sleep, because it would have brought on a fit if the patient had been awake. The epilepsy disappeared the ninth day after the vaccination. In him, who was afflicted with an hereditary epilepsy, and who was cured, vaccination was performed by incision, and the pustules were converted into an ulcer. Ten observers furnish various observations, four of which are detailed, and relate to periodical and obstinate fevers, such as quartans, double tertians, and quotidians. They were cured by vaccination. Two quotidians, with which young men of 28 were afflicted, had lasted for ten months; a double tertian, in a child of three years, had lasted three months. They ceased after vaccination. In four persons afflicted with intermittents, and vaccinated, the cow-pox appeared only upon one, and he alone was cured.

Several other observers, to the number of 14, have furnished various remarkable facts respecting different other diseases. In an infant, a year old, a palsy of the left arm, which had lasted two months, disappeared a month after vaccination, performed by making six punctures in the diseased arm. A great number of violent coughs have been suspended, moderated, or cured. The consequences of suppressed measles, namely, a dry cough, fever, and diarrhœa, were cured by a cow-pox induced by 20 punctures, during the suppuration of which a strong fever and miliary eruption occurred. A violent pain in the joint of the left thigh, with which a child of nine years of age was afflicted, with a threatening of spontaneous luxation of the limb, was treated by means of 18 punctures round the diseased joint. Sixteen pox, the aureolas of which were confluent, occasioned fever, and then suppurated. Soon after the pain of the joint disappeared, and the cure was complete. A white swelling of the knee in a child of eight years of age, and a deafness which had increased for 18 months in a child of six years of age, were both cured by vaccination.

Such are the facts which we have collected respecting the diseases existing at the time of vaccination, and cured by that process. We have noticed those only which are related with precision. We do not think that they ought to be always considered as cures due to vaccination. Separately taken, we do not see in them any thing else than a coincidence between the time of cure and vaccination; but taken collectively, we think that the number of facts, and the circumstances accompanying those which we have particularly noticed, give at least a presumption in favour of vaccination, more than sufficient to counterbalance the facts which have been alleged in favour of the small-pox, in what way soever that disease is communicated. We acknowledge, at the same time, that a comparison between vaccination and inoculation for the small-pox, in this point of view, cannot be fairly made, because a much greater number of cases of the former than of the latter have been given to the public. Vaccination, under the special protection of Government, has become the object of a regular and active correspondence, in which few facts have escaped observers, only in danger of being led astray by their zeal. Inoculation, on the other hand, but little favoured by Government, was become the object of enterprizes, in which a spirit of cupidity was much more prevalent than a spirit of observation.

It will be asked, perhaps, whether, if we admit an equality of advantages in favour of vaccination and inoculation, considered as a remedy for different diseases, it would not be of advantage to preserve the inoculation for the small-pox as a means of utility in certain situations.

We answer, that in such a comparison we ought not to leave out the dangers of a contagion, subtile and persevering like that of the small-pox, compared with the virus of the cow-pox, which can only be communicated immediately, because the least alteration destroys its properties. We ought also to reckon for something the hope at present entertained of being able to destroy the small-pox altogether. Could houses for inoculation, though established under the care of the police, be subjected to laws so severe, and to a sequestration so exact, as to prevent completely the spreading of the small-pox from them. Something might be said in its favour; but whoever considers the nature of man, and the state of society, must be convinced of the impossibility of securing any such object. In our opinion, even admitting vaccination and inoculation to be equally efficacious in removing other diseases, the balance in favour of vaccination is so strong that it is impossible to hesitate one moment about preferring it.

SIXTH QUESTION.

How far can we depend upon the preservative efficacy of the cow-pox, compared with the same advantage resulting from the small-pox, natural or inoculated? What consequences follow from this, properly considered, in the one or the other virus?

Nobody disputes the power of the cow-pox to preserve from the small-pox: and this question, which at the commencement was the most important of all, has now become only secondary to various others that have been put, and most of which we think we have already answered. At the same time, to this question must be referred a variety of other particulars of considerable interest, such, for example, as the distinction between the true and false cow-pox, the eruptions that have been confounded with the small-pox, the changes introduced in the bills of mortality by the introduction of the cow-pox, the hopes of destroying the small-pox, or of driving it out of the civilized world.

The idea of the faculty of preserving from the small-pox, divides itself into two questions. One may be thus stated: *Will an individual, after being vaccinated, if he be placed in a situation proper to produce the small-pox, and which usually produces it, continue exempt from that disease?* The solution of this question can only be obtained by a multitude of experiments; and that solution will give, then, not absolute certainty, but degrees of probability proportional to the number of experiments undertaken to resolve the question.

The other question is this: *Is it impossible for a vaccinated* *person to be infected with the small-pox?* Experience cannot decide, in the affirmative, the question when thus stated; but a single observation is sufficient to decide it in the negative. If that observation does not exist, the question must continue insoluble; because, in order to resolve it, we must be acquainted with the nature of the virus of small-pox and of cow-pox, with all the circumstances which are capable of excluding or producing contagion, and with the peculiar dispositions which prevent men from contracting it: all of them things absolutely unknown to us.

We must therefore confine ourselves to the first of these questions, and inquire into what confidence we may repose in the preservative power of the cow-pox. Such is the nature of the question to be resolved. We thought it necessary to fix its nature with precision, before proceeding to collect, as we have done with the other questions, the positive elements of its solution. Let us establish, in the first place, the nature of the facts which ought to constitute these elements.

It is obvious, in the first place, that we ought to exclude all those in which the characters of the cow-pox have not been ascertained. Some persons have considered the difference between the true and false cow-pox as a subtilty; but we answer, that when the characters, taken from the epoch of developement of the form and appearance of the pock, of the nature of the humour contained in it, of the manner of its desiccation, and of the mark which remains after it has dropped off, are so distinct from each other, as in the true and false cow-pox: when to this difference is joined the determination of the circumstances upon which the failure of vaccination usually depends, as, for example, the too late period at which the virus has been taken, the changes in the cow-pock which have occasioned the mixture of pus with the true limpid liquor of the cow-pock—when these circumstances have been accurately observed, no farther ambiguity remains, and the distinction between the two kinds of pock is perfectly established, and may be easily determined.*

* The facility with which cow-pock matter is altered by carriage, by exposnre to the air, by time, and the difficulty of preserving it, are known to every one. When diluted with water it loses its properties sooner. In Russia a cold of 0° destroyed its effects. In the experiments of Dr. Sacco, repeated each upon six children, and by 36 punctures, the virus diluted with water at 32° produced 28 pox; with water from 41° to 86°, 30 pox; with water at 122°, only 2; with gum-water, 30; with water containing some ammonia, 30; with saliva, 32. All other mixtures considerably diminished, or even destroyed the effect. When 24 punctures were made on different infants, the matter exposed to the air for five hours produced 22 pox; exposed to the air for 24 hours, 20 pox; exposed during three days, 15 pox. The contact of the other gases diminished the efficacy of cow-pox matter in five hours; but it was less injured by hydrogen, ammonia, azote, and carbonic acid, than by the others. Its efficacy was immediately destroyed by nitrous

This difference was established in consequence of errors committed in the first experiments. At Paris we were in possession of the false cow-pock matter, and were not acquainted with the effects of the true till Dr. Woodville made a journey to France, and naturalized among us the true matter.* At Geneva false cow-pock matter imposed upon the physicians, and disappointed their hopes during 21 months, till, in May, 1800, the virus sent by Dr. Pearson succeeded completely.†

The different characters of the true and false cow-pock matter have been already pointed out in the report inserted in the 5th volume of the Physical and Mathematical Memoirs of the Institute. They have been repeatedly published by the central committee of the Society of Paris; they are described in several parts of the Bibliotheque Britannique, and in various other publications. Dr. Sacco has given at the end of his work very good plates, where both the true and the false cow-poek are represented.

Besides this, Dr. Sacco, endeavouring to fix the time when the cow-pox may be usefully communicated, has determined by experiment the relation between the probability of success, and the successive days in which the virus has been collected. According to his observations, supposing that the cow-pock begins to appear on the third day, as usually happens, the success may be considered as certain if the virus be taken between the 5th and 8th day, reckoning from the time of the puncture; or between the 3d and 6th day, reckoning from the appearance of the pock. He found that when the matter was taken on the 6th day from the appearance of the pock, out of 100 punctures, 95 succeeded; when on the 7th, 92; when on the 8th, 88; when on the 9th, 85; when on the 10th, 80; when on the 11th, 50; and when on the 12th, only from 10 to 15. Besides this, the longer time elapses before the matter be extracted from a pock, the more likely is the pock to suppurate, and be converted into an ulcer. M. Sacco recommends, likewise, in order to be more certain of the efficacy of the matter, to avoid opening the pock too near the centre where the puncture was made, but to take the matter from as nearly as possible the outer edge of the pock, where it is more uniformly pure and limpid. Notwithstanding the various ingenious modes that have been contrived to transport the matter from one place to another, the most certain method of vaccinating, when it can be done, is to take the matter out of one arm, and immediately introduce it into another.‡

gas, muriatic and oxymuriatic acid gases. Light contributed to accelerate the alterations produced by the air.

* Report of the Central Committee for 1803, p. 12.

† Bibl. Brit. vol. xiv. p. 19; xv 76; xvi. 203.

‡ M. Voisin, physician at Versailles, whose zeal and talents have been

A second order of facts which ought to be excluded from the comparison, consists in observations of eruptive diseases, distinguished by the name of the small-pox, but which from their characters belonged evidently to the chicken-pox, or to some anomalous eruption, which have but a faint resemblance in form to the small-pox, but are in other respects quite different. Such eruptions show themselves every day upon children who have had the small-pox; and when they appear before that disease, they do not prevent it from infecting the patient. An attentive observer can easily distinguish such eruptions. The small-pox have a regular progress which cannot be mistaken; and when they are confluent they can be still confounded with other eruptions, which are usually exempt from all danger, and even from severe illness. Every observation, then, which does not give us the essential characters by which the small-pox is distinguished from other eruptive diseases, and in which we do not find the fever of the commencement of the disease, the eruption, the suppuration, the fever of intumescence which accompanies it, and the desiccation—every such observation cannot come into comparison with the observations in favour of the present question.

There is a third order of facts which cannot be admitted into the comparison of which we speak; we mean those cases in which a true small-pox makes its appearance during the time of vaccination, at an epoch when we must suppose that the infection was caught before the cow-pox could exert its preventive powers. This point has been discussed in the first report to the Institute. We have already, in the memoir, given several examples of it, in speaking of the eruptions and diseases ascribed to the cow-pox. On this point Dr. Sacco has made some curious experiments, to determine the precise time when the small-pox may still appear after vaccination. Supposing the cow-pox to appear on the 3d day after the puncture, the inoculation for the small-pox performed between the 1st and 5th day occasions the appearance of the small-pox between the 7th and 11th day. Inoculation performed on the 6th or 7th day occasioned a slight inflammation of the part punctured, without any general eruption. Either no pox appeared over the punctures, or if they did they speedily dried up. Inoculation performed from the 8th to the 11th day produced a slight alteration

justly appreciated by the central committee of Paris, who voted to him one of their medals, has ascertained that one of the best methods of preserving the cow-pock virus is that thought of by M. Bretonneau, to introduce it into capillary tubes, and then to seal them hermetically. He has succeeded, also, with the crusts, especially when fresh. But the success of these methods has never been so constant as when the matter is taken from one arm and introduced immediately into another.

at the place of the puncture, seldom a pock, or at least it very speedily dried up. Inoculation with small-pox matter being performed on 16 infants between the 11th and 13th day after vaccination, 3 of them only exhibited a slight redness at the place of the puncture, while the 13 others had no symptoms whatever. If the formation of the cow-pock be later than the 3d day, as happens sometimes, in that case the possibility of small-pox infection will be extended to a time proportionally longer.*

These details appeared to us necessary, in order to show to what degree of exactness observations on the preservative power of the cow-pox have been carried, and to show that the distinctions to which these researches have given origin are far from being, as some persons wish us to believe, subtilties and subterfuges invented to excuse the want of success.

Now in applying the remarks that have been made to the alleged observations of small-pox appearing after vaccination, if we exclude all those which want the conditions necessary for rendering them creditable, we find very little which can come in competition with the facts on the other side. There are, however, some, against which it is difficult to start any plausible objection. The Jennerian Society of London evidently admit the existence of such, in Articles 9, 10, 11, 14, and 15, of their Report. The College of Surgeons of London say, that out of 16,438 cases of vaccination there were 56, that is, 1 in 3,000, where it was insufficient to act as a preservative from the small-pox. But they have not informed us what was the immediate effect of these vaccinations, and to what circumstances their insufficiency could be ascribed. The authors of the Bibliotheque Britannique have inserted in their Work a letter from London, dated 5th August, 1811, stating that the National Cow-pox Establishment in London had published two cases of small-pox occurring after a most successful vaccination. "These cases," says the letter, "are well ascertained, and admitted on the part of the establishment. But they publish, at the same time, three cases of natural small-pox occurring twice in the same individual, after an interval of 11 years."†

The correspondence of the central committee of Paris contains some similar examples. Six observations were communicated by men well informed, and free from prejudice; but they were not accompanied with details sufficient to remove all uncertainty. Two of these announced small-pox appearing in the midst of an epidemic small-pox, which afflicted Beauvais in the autumn of 1810. But the children in whom this disease

* Trattato della Vaccinazione, p. 66. Bibl. Brit. vol. xlv. p. 63.
† Bibl. Brit. vol. xlviii. p. 168.

appeared had been vaccinated when the cow-pox was first introduced into France; and as no details are given, it is very possible that the disease communicated was the false cow-pox, at that time so common in this country. All the other children, vaccinated in the same place, and at later periods, continued exempt from the small-pox. There is a fact, which was verified by several members of the committee, and we ourselves saw the infant covered with a very numerous, but favourable small-pox, on the 7th December, 1806. This child, called Emma Kerouenne, lived in the old street of the Temple, No. 93, and had been successfully vaccinated on the 24th March, 1804, by M. Lanne, physician in Rue Français, who had preserved an account of the vaccination, and its progress. It is therefore evident that it is not impossible for a child that has been vaccinated to be afflicted with the small-pox. Nor indeed ought we to look for any such impossibility, as it has been well ascertained not to hold, even after inoculation, with the matter of small-pox.

But what degrees of probability do these observations leave, that vaccination will be a preservative from small-pox? We may obtain it by comparing the number of individuals who have taken the small-pox after vaccination with the whole number vaccinated, and who have not caught the infection, though repeatedly exposed to it. Another base of this evaluation is the number of counter experiments made, either by inoculation, or by placing persons that have been vaccinated in contact with those that are afflicted with the small-pox.

If we take the result of the correspondence of the central committee of Paris, the seven observations above-mentioned, supposing them all exact, are to be opposed to no fewer than 2,671,662 cases of vaccination. If it be objected that these seven observations, the only ones with which the committee were acquainted, are in all probability not the only ones which have occurred in the empire, we answer, that even these seven are not altogether free from uncertainty; and that the 2,671,662 vaccinations mentioned by the committee are far from being the whole number hitherto performed in France. These two numbers, being the whole obtained by the same means, are very fairly comparable with each other. They give us the ratio of 1 to 381,666.

With respect to counter-experiments, they are of three kinds: those made by inoculating with small-pox virus; those resulting from coming in contact with infected persons; those resulting from the reports of epidemic small-pox in villages, from which very few persons escape. The accounts transmitted to the committee present 640 individuals put to the test of inoculation; 680 persons living with individuals afflicted with the small-pox, and in contact with them, yet escaping the disease, while every other person took it; and 4312 who in the midst of epidemics affecting whole villages escaped the general contagion: making in all 5552 individuals that remained free from the contagion, in circumstances either artificial or natural, in which they ought, had it not been for vaccination, to have been afflicted with the disease.*

Similar results have been obtained in all other countries of Europe.

From all these facts, it is impossible not to conclude that the probability that vaccination will preserve from the small-pox is as strong as that inoculation with small-pox virus itself will prove efficacious; or that the small-pox will not recur a second time in the same individual: for it appears to us unreasonable, or at least premature, to conclude that small-pox will recur after the one oftener than the other.

If to these observations we join those which are their natural consequence, and which have been attested by physicians and magistrates, both in France and in other countries, that small-pox epidemics have been stopped in their progress by vaccination; that they have been excluded from those villages where vaccination had been generally practised; that these epidemics, which used to return at stated periods, have ceased to appear at their usual epochs; that several villages have ceased to know the small-pox, and that it has become much more uncommon than formerly in the great towns themselves, except in those places where the prejudices of the people have rejected vaccination; that the mortality of children has diminished, and that population has remarkably increased, in various places—if we consider all these circumstances, we shall not only appreciate the advantages which society is likely to reap from the precious discovery of Jenner, but the hope that the small-pox, that dreadful scourge of society, will disappear altogether, will be no longer chimerical; since this has been already realized in those places where the confidence of the people in the efficacy of vaccination has induced them generally to adopt it.

The reports published by the central committee of Paris in 1803, 1804, 1806, 1808, 1811, and 1812; and several bulletins of its correspondence, which have been successively published, contain numerous and positive proofs of all that we have ad-

* Report of the Central Committee in 1803, p. 103 to 168; in 1804, p. 26 to 34; in 1806, p. 47 to 60: in 1806 and 1807, p. 60 to 70; in 1808 and 1809, p. 60 to 67. Also notes communicated from the report of 1810. We may add that the motives which induced M. Chappon to retract his opinion were, that having seen during three years a great number of cases of small-pox, but not one case in those who had been vaccinated, *he was obliged to yield to the evidence.*

vanced; namely, of epidemics terminated or circumscribed, of their periodical returns prevented, by the number of vaccinations; and of the small-pox not only rendered rare, but of its being quite unknown in particular places, since the introduction of vaccination. The same phenomena are attested by the Minister of the Interior of the kingdom of Italy, especially in the epidemics observed at Brescia and Milan. The physicians of Geneva attest the annihilation of the small-pox in their town. The diminution of mortality, and the increased population, in consequence, have been ascertained at Rouen, at Creuznach, at Bezançon, in the departments of the Upper Rhine, of Dordogne, &c. and even in some quarters of Paris. These are irrefragable proofs of the advantages which may be expected from the discovery of Jenner.*

In the account which we have given to the Institute of the results obtained from the introduction of the cow-pox into France, after 12 years experience, we have only collected facts of undoubted authenticity. We were of opinion that the more advantageous the consequences, drawn from any observations, the more numerous they ought to be. We have rejected all the cases where the advantages resulting might be ascribed to peculiar circumstances in the case. It was not our intention to conceal any of the motives, or any of the facts, on which the objections made against vaccination have been founded. We have compared both sides of each question together; and we have aimed less at drawing absolute and exclusive consequences than at obtaining the requisite degree of exactness to determine as much as possible the measure of probability, and to estimate in consequence the value of the discovery, and the services which it may render to mankind.

We think we have established, in a satisfactory manner, that the virus of cow-pox does not introduce into the body any

* See the work of M. Dovillard, on the Influence of Small-pox on Population. Paris, 1806. Bibl. Brit. vol. xxxvi. p. 376.

A detail on the state of vaccination in the department of the Rhine and the Moselle, on the 1st January, 1809, by M. Lezay-Marnezia, prefect of the department. Bibl. Brit. vol. xlii. p. 182.

The details given by the same prefect in 1810 announce the disappearance of the small-pox in his department, and the increase of population by the vaccination of 9911 infants, almost the whole that were born within the year.

The details of the civil state of Rouen attest a diminution of the mortality of 4354 individuals in eight years, or more than 500 a-year.

The comparison made at Besançon by M. Barrey of the deaths during the last nine years, with those of the nine years preceding the establishment of vaccination, gives a diminution of 1619 individuals, all in the first ten years of life.

In the department of Dordogne M. le Baron Maurice, Prefect of the department, announces in six years an excess of births above deaths which amounted to 4449 for 1810, and to 22,007 for the whole ten years.

matter calculated to produce disagreeable effects, and which ought to be thrown out by eruptions similar to those of the small-pox:

That the eruptions, which at first frequently followed vaccination, were owing not to the nature of the virus itself, but to other circumstances, most of them well known and easily determinable, during the existence of which the cow-pock matter was applied:

That the unfortunate results of vaccination sometimes observed ought to be ascribed to causes foreign to vaccination, which have made their appearance during its course, or which, having previously existed, acquired an intensity which ought to be ascribed not to the virus of cow-pox, but to the peculiar state of the subjects vaccinated:

That the disorders which have been sometimes observed to follow vaccination, when they are not owing to diseases already existing, are evidently particular cases, owing to the condition of individuals, and which bearing no proportion to the number of cases exempt from all such disagreeable results, can give no room for drawing a general and unfavourable conclusion:

That these observations, even supposing them incontestable, are more than compensated by the numerous examples of chronic and obstinate maladies which have been completely and unexpectedly cured by vaccination: and that these examples, if we compare them with similar examples in favour of small-pox inoculation, if to this comparison we join the differences in the essential character of the two species of virus, and in their contagious effects, give to vaccination an incomparable advantage over small-pox inoculation, considered as a preventative of small-pox, and as a remedy for other diseases:

Finally, that the preservative effect of the cow-pox virus, when this virus is pure, and has produced genuine cow-pox, is at least as certain as that of the virus of small-pox itself; and that when considered relative to society in general, vaccination has an advantage which small-pox inoculation cannot possess; namely, the advantage of stopping, diminishing, and destroying epidemic small-pox; of diminishing the mortality of children, and of increasing the population; and that the results already obtained give hopes of seeing the small-pox, one of the most dismal diseases under which mankind has groaned, removed entirely from the face of the earth.

BERTHOLLET, PERCY, HALLE, Reporter.

The Class approves of this Report, orders it to be immediately printed, and to be inserted in the next volume of Memoirs.

Sept. 7, 1812. G. CUVIER,

Article VII.

Analysis of Graphite. By Apothecary Schrader.*

That graphite is a combustible body, composed almost entirely of charcoal, has been long known from the experiments of Scheele, who first pointed out the method of distinguishing it from molybdena. His experiments were confirmed by the subsequent investigations of Berthollet, Monge, and Vandermonde. These gentlemen put graphite into a glass jar, filled with oxygen gas, and exposed in the focus of Tchirnhouse's burning-glass. The graphite was speedily consumed, and there remained behind a quantity of iron which had been melted into grains by the heat. Scheele had likewise obtained a residue of iron when he burnt graphite with ten times its weight of saltpetre.

In these experiments it was observed that the graphite endured a much higher temperature, and required more oxygen for its complete combustion, than the same weight of common charcoal. It was believed, in consequence of the experiments of Guyton, that the carbon in graphite was less oxydized than the charcoal of vegetable bodies. The three above-named French chemists were of opinion that graphite does not burn so easily as charcoal, because it consists of carbon and iron chemically combined together. They conceived that the iron was in the metallic state, and that the carbon was completely saturated with iron. Thus carbon and iron were considered as the constituents of graphite; but it does not appear that any person thought of carefully examining the nature of the ferruginous residue that remains behind after the combustion of graphite.

I had a particular reason for endeavouring to ascertain all the substances that existed in Spanish graphite, and among other substances I found copper in it. In order to determine whether the English graphite would yield the same constituents I made the following experiments:—

A.

In the first place, I picked out of my own collection several very pure specimens of English graphite. From these I obtained the following results:—

1. As the Spanish graphite had contained a good deal of sulphate of iron and of copper, which could be separated by boiling the mineral in water, I boiled 100 grains of English

* From Der Gesellschaft Naturforschender Freunde zu Berlin Magazine for 1810, p. 205.

graphite reduced to a fine powder in a sufficient quantity of water. The liquid treated with acetate of barytes yielded $\frac{1}{4}$ of a grain of sulphate of barytes, which may be considered as equivalent to $\frac{3}{10}$ths of a grain of sulphate of iron.*

2. The graphite thus boiled in water was burnt in a silver crucible with 10 times its weight of saltpetre. The combustion always took place after the mass had continued for some time red hot. The residuary mass, after the salt had been washed off by water, weighed 16·2 grains, and was a light reddish grey powder. The saline ley was evaporated to dryness, again dissolved in water, saturated with an acid, and evaporated a second time. By this treatment some silica separated. Carbonate of soda separated $\frac{9}{10}$ths of a grain of alumina.

3. The grey powder was boiled in nitro-muriatic acid, which assumed in consequence a yellow colour. There separated from this solution a white precipitate, which swam in curds in the liquid, and which when strongly dried weighed 6·2 grains.

4. The yellow solution, which by dilution with water had let fall a white powdery precipitate, was super-saturated with caustic ammonia, and the solution evaporated. No distinct blue colour appeared. The liquid was mixed with carbonate of soda, and exposed to heat till the whole of the ammonia was driven off. The salt thus obtained being dissolved in water, left a greyish green residue, part of which was taken up by ammonia; and from that solution prussiate of potash threw down a brownish red precipitate, indicating clearly the presence of copper.

5. The ferruginous precipitate obtained in paragraph 4, was, while still moist, boiled in a dilute alkaline ley, by which 1·6 grain of alumina was separated. The oxide of iron, being well washed, and heated with oil, gave 5·9 grains of iron completely attracted by the magnet.

6. I took it for granted that the silica, whose existence in graphite I had already ascertained by previous experiments, would be found in the 6·2 grains, which separated in paragraph 3, and in the powder which fell from the ferruginous solution. These were accordingly mixed with an alkaline ley in a platinum crucible, and exposed to a red heat. The mass was completely dissolved in water. It was saturated with muriatic acid, and again evaporated. Instead of leaving, as is usual when silica is present, a gelatinous mass behind, a fine sandy precipitate separated, which being examined by a magnifying glass appeared to consist of small crystals. This sandy precipitate was dissolved in soda ley without the assistance of heat, and the solution assumed a slightly brown colour. The ley was saturated with muriatic acid, and evaporated. The silica now separated in the form of a jelly; and the filtered solution, when treated with prussiate of

* A hundred grains of crystallized sulphate of iron yielded, when precipitated by muriate of barytes, 82·7 grains of sulphate of barytes.

potash, gave an olive green precipitate; and with tincture of nutgalls, a brownish red precipitate; indicating the presence of titanium.

I had already suspected the presence of titanium in graphite, in consequence of a previous set of experiments. Some grains of silica, which appeared to contain titanium, which I had obtained in a set of very different experiments, were gently digested in muriatic acid. Some titanium was found, in consequence, in the solution. The white residue, which did not dissolve, was mixed with water, and set aside one night to spontaneous evaporation. Next morning the liquid was found transparent, but of an indigo blue colour; and immediately over the white powder was a layer of flocks of an indigo blue colour. I prosecuted this appearance, and tried if any molybdenum or tungsten could be detected; but I could find no traces of either, nor of any other metal, except titanium. I have not since remarked this appearance, in consequence f similar trials. That titanium enters into combinations with alkalies and earths is already known, in consequence of the experiments of Vauquelin, and is fully established by my experiments on graphite. The titanium separates itself in this case in various states and appearances. I must on that account give a statement of my various experiments to separate it from other bodies, and estimate its quantity.

B.

For the farther prosecution of these experiments on the proportion of titanium contained in graphite, I made choice of a specimen of compact English graphite from another collection. This specimen when boiled in water yielded nothing remarkable to that liquid, only that the water in which it had been boiled had a strong clayey taste.

1. Two hundred grains of graphite, that had been thus boiled, were burnt in a silver crucible, with ten times their weight of saltpetre. The residue weighed 32·2 grains.

2. This residue was boiled in nitro-muriatic acid. When the liquid was diluted with water, one grain of a white powder fell, which when strongly dried became bluish.

3. It was, together with the residue left by the nitro-muriatic acid, heated to redness in a platinum crucible with an alkaline ley. The mass was dissolved in water, and treated with muriatic acid. By this treatment 7·2 grains of silica and 3·4 grains of oxide of titanium were separated.

4. The solution obtained in paragraph 2 was saturated with caustic ammonia. The colourless solution was evaporated, mixed with a sufficient quantity of carbonate of soda, evaporated to dryness, and strongly heated, in order to get rid of the ammonia. It exhibited, before the whole of the ammonia was driven off, a

slight shade of blue. The residue being exposed to a strong heat, and re-dissolved in water, there fell 2 grains of a greyish powder, which, by a method to be described afterwards, I separated into 0·2 grain of oxide of copper, 1·3 grain of oxide of titanium, and 0·5 grain of oxide of iron.

C.

Partly in order to estimate these constituents with still greater precision, and partly to ascertain the composition of English graphite with still more accuracy, I undertook the examination of another specimen of English compact graphite, which I knew for certain to have been obtained out of the mine of Barrowdale.

1. This graphite was reduced to a fine powder, and boiled with water. The liquid continued colourless, but acquired a strong taste of clay. It produced no change upon litmus paper, and prussiate of potash produced no perceptible change in it. With tincture of nutgalls it exhibited a slight tendency towards a reddish brown colour, which was increased by the addition of carbonate of soda, and at last passed into greenish brown. No precipitate fell. Caustic ammonia produced no alteration in it. Muriate of barytes rendered it opalescent, and slightly muddy. The muddiness was not removed by nitric acid; but the precipitate that fell was so small that it could not be weighed. Nitrate of silver likewise occasioned an opalescence; but no precipitate fell.

2. Two hundred grains of the graphite thus boiled in water were burnt in a silver crucible with ten times their weight of saltpetre. A considerable time elapsed before the combustion took place. After the saline residue had been washed away in water, there remained behind 34·4 grains of a light brown powder. From the saline solution, by means of an acid and carbonate of soda, 1 grain of alumina was separated. It was still mixed with a small quantity of oxide of iron.

3. The light brown powder was boiled in nitro-muriatic acid. A portion remained undissolved, which had a greyish colour; but in other respects resembled silica. This residue, while still moist, was boiled in an alkaline ley. A brownish black powder soon separated from it, which weighed 3·6 grains.

4. The acid solution of paragraph 3 was diluted with water. A white powder separated, which weighed 0·2 grain. This was mixed with the 3·6 grains of the brownish black powder of paragraph 3, and heated to redness in a platinum crucible with a little carbonate of soda. The whole dissolved in muriatic acid, and the solution had a greenish yellow colour. The solution being neutralized with ammonia, and treated with prussiate of potash and tincture of nutgalls, exhibited the appearances produced by a solution of titanium.

5. The alkaline ley from which the brownish black powder of paragraph 3 had separated, was supersaturated with muriatic acid, and evaporated. It became in consequence gelatinous. The silica being separated, and heated to redness, weighed 7 grains.

6. The diluted acid solution of paragraph 3 was precipitated by means of caustic ammonia. The precipitate, while still moist, was boiled with caustic soda, which took up 3·6 grains of alumina.

7. The ferruginous precipitate which had been boiled in the alkaline ley and edulcorated, was dried; and being mixed with the oxide of iron obtained in paragraph 8, was rubbed into a paste with oil, and exposed to a red heat. There remained 11·6 grains of black oxide of iron, which was completely attracted by the magnet.

8. The ammoniacal ley, from which the iron had been precipitated in paragraph 5, was entirely colourless. It was concentrated by evaporation; but still acquired no colour. It was mixed with a sufficient quantity of potash, evaporated to dryness, and exposed to a heat sufficient to drive off the whole of the ammonia. The dry salt had a greenish yellow colour. When it was dissolved in water a blackish brown powder separated, which weighed 4·6 grains, and which being digested with ammonia neither assumed a blue colour, nor did prussiate of potash indicate that it contained any copper. Farther experiments convinced me that this residue contained titanium. It was separated in the following manner:—

The powder was heated to redness with saltpetre in a platinum crucible. The saline residue being dissolved in water, no perceptible portion of the powder was taken up. Its colour was now rather reddish than brown. It was gently digested with very dilute muriatic acid, which acquired a yellow colour, and a blackish grey powder still remained undissolved. The muriatic solution being treated with caustic ammonia, oxide of iron fell, which was mixed with that of paragraph 7. The ammoniacal solution, which was colourless, being neutralized, and mixed with prussiate of potash, assumed a light reddish colour, with a distinct opalescence. This colour might perhaps originate from a trace of copper, so small that its quantity could not be estimated. Whence the shade of white mixed with the red proceeded it is impossible to say. It might perhaps be owing to the presence of a small portion of alumina.

The blackish grey residue which the muriatic acid had not dissolved weighed 2·5 grains, and was completely dissolved by nitro-muriatic acid. The solution had a greenish yellow colour. Tincture of nutgalls threw down from it a brownish red precipitate; and prussiate of potash, a precipitate which had a dirty greenish colour. The leek-green colour could be distinguished, and indicated titanium, though still probably combined with a small residue of iron.

In this manner I found that 200 grains of this graphite, after being boiled in water, and burnt with saltpetre, left a residue which consisted of

Black oxide of iron	11·6 grains.
Silica	7·0
Alumina	4·6
Oxide of titanium	6·3
	29·5

The graphite previously examined had exactly the appearance of this last specimen. It follows from it that English graphite is not quite free from copper. The two specimens differed a little from each other in their specific gravity.

That of the first was	2·250
That of the second	2·320

When we boil English graphite in nitro-muriatic acid, nitric acid, or muriatic acid, these acids dissolve ten per cent. of the constituents of graphite which are not charcoal. This separation of these constituents from the charcoal in this way is rather against the supposition that the charcoal is chemically combined with the iron, or with any of the other constituents. The charcoal of wood exhibits very different properties. Whether in this last the charcoal is already oxydized, as it seems to be in graphite, or whether, as Berthollet supposes, it be united to hydrogen, are quite different questions. The Spanish graphite frequently contains foreign bodies in distinct masses. These are sometimes crystallized. When we boil it in water, we obtain a solution of sulphate of iron and sulphate of copper. When the graphite is burnt with saltpetre, we frequently find traces of chromium in it. Besides these foreign substances, I have likewise found iron pyrites in this graphite. It is not improbable that it contains likewise copper pyrites. The only reason for suspecting the accuracy of this supposition is the small proportion of copper obtained when the graphite is burnt, and the residual ashes analysed.

I boiled pounded Spanish graphite in water, in order to remove the soluble salt, and burnt 200 grains of it with ten times its weight of saltpetre in a silver crucible. The combustion did not take place till the heat had been continued for a considerable time. But as soon as the powder was introduced, there immediately appeared on its surface the greenish blue flame which characterizes pyrites.

The saline ley obtained by washing the residue of the combustion being mixed with muriate of barytes, gave 46 grains of sulphate of barytes.* The reddish grey powder which separated from the saline ley was treated in the same manner as the residue of the English graphite. The constituents obtained by this treatment from 200 grains of Spanish graphite were as follows:

Black oxide of iron (derived from the pyrites) ..	14·2 gr.
Silica	3·0
Alumina	2·4
Oxide of copper	1·0
Oxide of titanium	3·1
	23·7

Article VIII.

Astronomical Observations at Hackney Wick. By Col. Beaufoy.

Latitude 51° 32′ 40″ North. Longitude West in Time 6·82″.

1813.

	Hackney Wick.			Greenwich Time. *			
Feb. 24, emersion of Jupiter's 1st satellite..	8h	36′	20″	8h	36′	13″	18‴
Mar. 5, emersion of ditto 2d ditto ..	7	53	01	7	52	54	18
March 6, immersion of μ Cœti per moon ..	8	50	17	—	—	—	—
March 6, emersion of ditto	9	43	17	—	—	—	—
March 7, emersion of Jupiter's 3d satellite	8	08	25	8	08	18	18
March 8, immersion of Aldebaran, not observed	—	—	—	—	—	—	—
March 8, emersion of ditto	7	29	47	—	—	—	—
11th Ditto, Civil time 11, emersion of Jupiter's 1st satellite	12	25	49	12	25	42	18
March 12, emersion of ditto, 1st ditto	6	55	07	6	55	00	18
March 12, emersion of ditto, 2d ditto	10	30	06	10	29	59	18

These observations are set down in mean time, and were made under favourable circumstances, with one of Dollond's five feet refracting telescopes, with a magnifying power of about 80 times.

The moon's age being five days, and the non-illuminated part of her disc extremely well defined, particular attention was paid to ascertain if at the immersion of μ Cœti the star had its light diminished, but no such effect took place.

Rain, between noon the 1st of February and the 1st March, 2·200 inches.

* I burnt 100 grains of compact pyrites from *Katharine*, in Raschau, with an excess of saltpetre, and obtained from the residue 206·6 grains of sulphate of barytes. According to this estimate, the 46 grains of sulphate represent $22\frac{16}{62}$ of pyrites.

* I have applied the Longitude with the wrong sign – instead of + Should have been
h ′ ″
8.36.26.22
7.53.07.22
8.08.31.[illegible]
12.25.55
6.55.1[illegible]

Article IX.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *New Salts of Lead.*

Chevreul has lately examined some salts of lead which had been originally discovered by M. Proust, and in which he had conceived that there existed an oxide containing less oxygen than the yellow oxide. I afterwards examined the same salts, and found that they contained the common yellow oxide. Chevreul has found that the acid combined with the oxide in these salts is not, as we had supposed, the *nitrate*, but the *nitrous* acid. This is what distinguishes them from the nitrates of lead formerly known. I shall here give a sketch of the results obtained by Chevreul.

1. There are two nitrates of lead. 1. The common octahedral nitrate, which is a supernitrate, containing an excess of acid. 2. The neutral nitrate, composed of scales. It may be formed by boiling together a mixture of yellow oxide of lead and octahedral nitrate.

The supernitrate of lead is composed of

Acid	33	100
Yellow oxide	67	203

The scaly or neutral nitrate is composed of

Acid	19·86	100
Yellow oxide	80·14	403

So that the quantity of oxide in the neutral salt is twice as great as in the supersalt.

2. Three hundred and fifty parts of water, 4 parts of supernitrate of lead, and 6 parts of lead, were boiled in a flask for 14 hours: 5·4 parts of the lead were dissolved. The liquid became at first yellow, but the yellow colour gradually disappeared, and a greyish white matter precipitated. The glass was attacked. The grey matter consisted of a mixture of silica and hydrate of lead. The lead amounted to 0·47 of a part of yellow oxide.

3. The liquid deposited needle-form crystals, which weighed 5·95. The mother water contained nitrate of potash.

4. By this process the nitric acid is decomposed, and converted into nitrous acid and nitrous gas. Two salts are formed, one in plates (a nitrite), and one in needles (a subnitrite).

5. The nitrite is best prepared by causing a current of carbonic acid gas to pass through the solution of the subnitrite.

6. The nitrite possesses the following properties: It is little soluble in cold water; but boiling water dissolves nearly one-tenth of its weight of it. It was decomposed by all the acids tried. Even carbonic acid partially decomposes it, converting it into subnitrite. Its constituents are,

Acid	18·15	100
Yellow oxide	81·85	450

7. The subnitrite possesses the following properties. The colour is reddish yellow. It crystallizes in needles: 100 parts of boiling water dissolve about three parts of it, and retain about one part when cooled down to 73°. It is composed of

Acid	9·9	100
Yellow oxide	90·1	910

So that it contains double the quantity of oxide that exists in the nitrite. See Ann. du Museum d'Histoire Naturelle, N° III. p. 1888.

There is one circumstance in the above statement obviously inaccurate. Chevreul says, that by the action of the lead the nitric acid was resolved into nitrous acid and nitrous gas. Such a resolution is impossible; because nitric acid contains more oxygen than exists in nitrous acid and nitrous gas. The meaning, however, may be made out, though it is obscurely expressed. What Chevreul must mean is obviously this: the nitric acid gave out a portion of its oxygen to the lead, and the remainder was resolved into nitrous acid and nitrous gas—an effect which might very well happen.

II. *New Properties of Light.*

In our last number we gave a short summary of the new experiments on light made by Dr. Brewster, and likewise of what had been done on the same subject by Biot and Arrago in France; but we have reason to believe that a more particular explanation of some of the points will be acceptable to our readers.

The double refraction of light by certain bodies has occupied the attention of philosophers from the first observation of the phenomenon by Bartholine and Huygens, down to our own time; but no satisfactory explanation of it has been offered. Even Newton contributed but little to the elucidation of this difficult subject. Nor is the late effort of Laplace such as corresponds with his well-earned celebrity, and with his eminence as a mathematician. The phenomena of double refraction are as follows:—

If a ray of light fall upon one of the surfaces of a rhomboid of Iceland crystal, and is transmitted through the opposite surface, it is separated into two pencils, one of which proceeds in the direction of the incident ray, while the other forms with it an angle of 6° 16′. The first of these pencils is said to experience the *usual* or *ordinary* refraction, and the other the *unusual* or *extraordinary* refraction. If the luminous object from which the ray of light proceeds be looked at through the crystal, two images of it will be distinctly seen, even when the rhomboid is turned round the axis of vision. If another rhomboid of Iceland spar is placed behind the first, in a similar position, the pencil refracted in the ordinary way by the first will be so also by the second, and the same thing holds with the extraordinarily refracted pencil—none of the pencils being separated into two, as before. But if the second rhomboid is turned slowly round, while the first remains stationary, each of the pencils begin to separate into two; and when the eighth part of a revolution is completed, the whole of each of the pencils is divided into two portions. When the fourth part of a revolution is completed, the pencil refracted in the ordinary way by the first crystal will be refracted in the extraordinary way only by the second, and the pencil refracted in the extraordinary way by the first will be refracted in the ordinary way only by the second; so that the four pencils will be again reduced to two. At the end of $\frac{3}{8}$, $\frac{5}{8}$, and $\frac{7}{8}$, of a revolution, the same phenomena will be exhibited as at the end of $\frac{1}{8}$ of a revolution. At the end of $\frac{4}{8}$ and $\frac{6}{8}$ of a revolution, the same phenomena will be seen as at the first position of the crystals, and at the end of $\frac{8}{8}$ of a revolution.

If we look at a luminous objeet through the two rhomboids, we shall at the commencement of the revolution see only two images, viz. one of the least, and of the greatest refracted images. At the end of $\frac{1}{8}$ of a revolution four images will be seen, and so on as in the preceding example.

It is obvious that the light which forms these images has suffered some new modification, or acquired some new property, which prevented it in particular parts of a revolution from penetrating the second rhomboid. This property has been called *polarization*; and light is said to be *polarized* by passing through a rhomboid of calcareous spar, or any other doubly refracting crystal.

Almost all crystallized substances possess the property of double refraction, and consequently the power of polarizing light. The most important of these, arranged in the order of their refractive power, according to the experiments of Dr. Brewster, are the following:—

1. Chromate of lead.
2. Carbonate of lead.
3. Zircon.
4. Pistazite.
5. Carbonate of strontian.
6. Chrysolite.
7. Calcareous spar.
8. Topaz.
9. Tartaric acid.
10. Rock crystal.
11. Sulphate of copper.
12. Selenite.
13. Sulphate of iron.

Some years ago Malus, a colonel of engineers in the French army, announced the discovery of a new property of reflected light. He found that when light is *reflected at a particular angle* from all transparent bodies, whether solid or fluid, it has acquired by reflection that remarkable property of polarisation, which had hitherto been regarded as the effect only of double refraction.

If the light of a taper, reflected from the surface of water at an angle of 52° 45′, be viewed through a rhomboid of Iceland crystal which can be turned about the axis of vision, two images of the taper will be distinctly visible at one position of the crystal. At the end of $\frac{1}{8}$ of a revolution one of the images will vanish, and it will re-appear at the end of $\frac{2}{8}$ of a revolution. The other image will vanish at the end of $\frac{3}{8}$ of a revolution, and will re-appear at the end of $\frac{4}{8}$; and the same phenomena will be repeated in the other two quadrants of its circular motion. The light reflected from the water therefore has evidently been *polarized*, or has received the same character as if it had been transmitted through a doubly refracting crystal.

The angle of incidence at which this modification is superinduced upon reflected light increases in general with the refractive power of the transparent body; and when the angle of incidence is greater or less than this particular angle, the light suffers only a partial modification, in the same manner as when two rhomboids of Iceland spar are not placed either in a similar or in a transverse position.

Malus found that light reflected from opaque bodies, such as black marble, ebony, &c. was also polarized. But polished metals, according to him, did not impress that property, though they did not alter it when it had been acquired from another substance. Dr. Brewster, however, has observed, that polished metals polarize light as well as other substances.

When a ray of light was divided into two pencils by a rhomboid of Iceland spar, Malus made these pencils fall on a surface of water at an angle of 52° 45′. When the principal section of the rhomboid (or the plane which bisects the obtuse angles) was parallel to the plane of reflection, the ordinary pencil was partly reflected, and partly refracted, like any other light; but the extraordinary ray penetrated the water entire, and not one of its particles escaped refraction. On the contrary, when the principal section of the crystal was perpendicular to the plane of reflection, the extraordinary ray was partly refracted and reflected, while the ordinary ray was refracted entire.

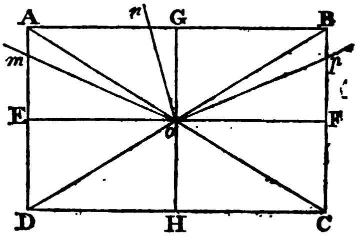
While Dr. Brewster was employed in repeating the experiments of Malus, and observing the effect produced upon light by transmitting it through transparent and imperfectly transparent bodies, he was struck by a singular appearance of colour in a plate of agate. This plate, bounded by parallel faces, was about the 15th of an inch in thickness, and was cut in a plane perpendicular to the laminæ of which it was composed. This agate was very transparent, and gave a distinct image of any luminous object. On each side of this image was one highly coloured, forming with it an angle of about 10°, and so deeply affected with the prismatic colours that no prism of agate, with the largest refracting angle, could produce an equivalent dispersion. Both the coloured images and the colourless image were found to be polarized. Dr. Brewster found that when the image of a taper, reflected from water at an angle of 52° 45′, is viewed through a plate of agate, having its laminæ parallel to the plane of reflection, it appears perfectly distinct; but when the agate is turned round, so that its laminæ are perpendicular to the plane of reflection, the light which forms the image of the taper *suffers total reflection, and not one ray of it penetrates the agate.*

He found likewise that if a ray of light incident upon a plate of agate be received after transmission upon another plate of the same substance, having its laminæ parallel to those of the former, the light will find an easy passage through the second plate; but if the second plate has its laminæ perpendicular to those of the first, the light will be *wholly reflected, and the luminous object will cease to be visible.*

But the most curious observation made by Dr. Brewster on the agate is the presence of a faint nebulous light, unconnected with the image, though always accompanying it, lying in a direction parallel to the laminæ. This unformed light never vanishes along with the images; and in one of the specimens of agate it is distinctly incurvated, having the same radius of curvature with the adjacent laminæ. Dr. Brewster found the same property in the carnelian and chalcedony, minerals of which the agate is usually composed. Dr. Brewster ingeniously conjectures that the structure of agate is an approach to that particular kind of crystallization which occasions double refraction, and that the nebulous light is an imperfect image arising from that imperfection of structure. He conceives that the phenomena of double refraction are produced by an alternation of laminæ of two separate refractive and dispersive powers. Thus in calcareous spar, one set of laminæ may be composed of lime, the other of carbonic acid. The only double refracting crystal incompatible with this supposition is sulphur, which, however, may hereafter be ascertained to be a compound.

Another very singular discovery of Dr. Brewster is, that when polarized light is transmitted through certain transparent bodies, it is unpolarized by these bodies in certain positions, and unaltered by them in others. The transparent bodies which possess this property are *rock crystal, topaz, chrysolite, borax, sulphate of lead, felspar, selenite, citric acid, sulphate of potash, carbonate of lead, leucite, tourmaline, pistazite, mica, Iceland spar, agate without veins,* some pieces of *plate glass.* Gum arabic, horn, glue, and tortoiseshell, depolarize light in every position.

Dr. Brewster has observed that mica and topaz exhibit some singular phenomena with light. Let the rectangle A B C D represent a plate of mica. When a prism of calcareous spar is placed in a vertical, or a horizontal line, upon this plate, *polarized* light viewed through them both suffers no change. The horizontal and



vertical lines E F, G H upon the plate of mica may be called the neutral axes of the mica. When the Iceland spar is placed in the diagonals A C, B D of the plate, the polarized light is *depolarized,* and hence these diagonals may be called *depolarizing axes.* If we examine a polarized image by the prism of Iceland spar, placed upon the *vertical neutral* axis of the mica, the polarity of the light will of course continue, and only one image will be seen; but if we incline the plate of mica forwards, so as to make the polarized light fall upon it at an angle of about 45°, the image that was formerly invisible starts into existence, and therefore the light from which it was formed has been depolarized. If the same experiment is made upon the *horizontal neutral* axis, no such effect is produced; and hence it follows that the *vertical neutral* axis is accompanied by an *oblique depolarizing* axis. By making the same trials with the *depolarizing* axes, it will be found that each is accompanied by an *oblique neutral* axis; and therefore each plate of mica possesses *two oblique neutral* axes, and *one oblique depolarizing* axis. The oblique depolarizing axis is represented by the line *o n,* and the two oblique neutral axes by the lines *o m* and *o p.* The angles G *o n,* G *o m,* G *o p,* being about 45°, and the planes of these angles being perpendicular to the plate of mica. Topaz was found to exhibit the same phenomena to a limited extent; but no other substance tried.

We have still some other discoveries, made by Dr. Brewster, on light, to mention; but this article has already extended to such a length that we must delay our account of them till our next number.

III. *Matches that take fire when dipped into sulphuric acid.*

In answer to our Weymouth correspondent, who requests us to inform him what is the composition of these matches, we answer, that such matches are by no means new; they have been known for many years to chemists. They are composed of the hyperoxymuriate of potash in powder, mixed with sugar or charcoal powder. The whole must be well mixed together. But whoever makes them must beware of rubbing them hard in a mortar, for such a mixture is apt to explode. There is no occasion for much nicety about the proportions. I have often made such matches, eight or ten years ago, and then employed equal weights of the two ingredients.

IV. *Population of Sunderland.*

A correspondent from Sunderland has obliged us with a correction of our statement of the population of that town in our last number. It consists, he says, of three parishes united, namely, Sunderland, Bishopwearmouth, and Monkwearmouth; and the population of all the three, which constitutes the town of Sunderland, exceeds, he says, 30,000. We have not the parliamentary returns for 1811 at hand; but have no doubt of the accuracy of our correspondent.

V. *Geognosy of Werner.*

In our last number we inserted an admirable paper by Professor Jameson, of Edinburgh, vindicating the geognosy of Werner from the attack made upon it by the Edinburgh Review. Professor Jameson maintained, against the opinion of the reviewer, that Haüy and Brogniard, in their account of the environs of Paris, had adopted the conclusions, and used the language, of the Wernerian geognosy. Had he seen the *Recherches sur les Ossemens fossiles de Quadrupeds*, published by Haüy in 1812, he would have found the following passage, which deserves to be quoted as a vindication, or rather demonstration, of Professor Jameson's opinion :—" En effet, la partie purement minerale du grand probleme de la theorie de la terre a eté etudiée avec un soin admirable par de Saussure, et portée depuis à un developement etonnant par M. Werner et par les nombreux et savans eleves qu'il a formés—Le second (Werner) profitant des nombreuses excavations faites dans le pays du monde ou sont les plus anciennes mines, a fixé les loix de succession des couches; il a montré leur ancienneté respective et pursuivi chacune d'elles dans toutes ses metamorphoses. C'est de lui, et de lui seulement, que datera la geologie positive, en ce qui concerne la nature minerale des couches; mais ni l'un ni l'autre n'a donné à la determination des especes organisées fossiles, dans chaque genre de couche, la rigueur devenue necessaire, depuis que les animaux connus s'elevent a un nombre si prodigieux." Tome i. p. 34.

Article X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 25th of February the following papers were read:—

An account of a new micrometer, by Dr. W. Hyde Wollaston. It consists of an instrument similar in appearance to a common telescope, with three sliding tubes. At one end there is a spherical lens, with a focus of about $\frac{1}{13}$th of an inch; and within $\frac{1}{13}$th of an inch of it, there is a slit, through which objects can be seen. Instead of the object-glass there are a number of parallel wires placed beside each other, of a determinate diameter, as an object of comparison. Within are two glasses, between which the object to be measured is put. Its size is determined by comparing the magnified image with the standing wires at the extremity of the tube, and ascertaining their distance from the eye when both appear of a size: 16 inches distance in the instrument corresponds to a wire of $\frac{1}{10000}$ of an inch in diameter, and 8 inches to one of $\frac{1}{5000}$th of an inch.

A paper on the winter solstice, by Mr. Pond, Astronomer Royal. He found the obliquity of the ecliptic at the summer solstice to be 23° 27′ 51·5″, and at the winter solstice 23° 27′ 47·37″. The difference he conceives to depend upon refraction. He is endeavouring at present to ascertain whether Dr. Bradley's allowance for refraction be correct.

A paper on the black matter in the glands of the lungs in old persons, by Dr. Pearson. The lungs are at first light coloured; but they become mottled about the age of 20, gradually increase in darkness, and in old persons are nearly black. Dr. Pearson examined the cause of this change, and found it owing to a quantity of charcoal contained in the glands. This charcoal, he conceives, is taken in with the breath, suspended in the air. He accounts for its absence in young persons, and in brute animals, by the state of the lymphatics; by means of which, he conceives, the charcoal makes its way into the bronchial glands.

On Thursday the 4th of March a paper by the late Mr. Kirby Trimmer was read, on the fossils found in the neighbourhood of Brentford. This town lies on the side of the Thames, about six miles west from London. The soil, as far as it has been dug, consists of five distinct beds. The uppermost, is a gravelly loam; the second, sand and gravel; the third, a calcareous loam; the fourth, sand; and the fifth, blue clay, which passes under London, and is found every where in the neighbourhood. The thickness of the clay bed is 200 feet, reckoning from the surface of level ground. Its depth on hilly grounds is greater: thus Lord Spencer, at Wimbleton, was obliged to dig a well 530 feet deep, before he got through the clay. The uppermost bed contains no fossil remains whatever. The next three contain the tusks of elephants, both African and Indian, of the hippopotamus, the horns and jaws of oxen, the horns of deer, snail-shells, and the shells of fresh-water fish; but no sea animals. The clay contains the fossil remains of sea animals alone; as echini, shells, &c. These fossils are scattered without order in the beds, and the bones must have been deposited long after the death of the animals, for no two are found contiguous, in the order in which they existed in the living animal.

On Thursday the 11th of March there was read a description of a glass apparatus for condensing gases, by Mr. Austin. This was a modification of a condensing apparatus formerly contrived by Mr. Austin, the description of which was published in one of the volumes of the Memoirs of the Royal Irish Academy. It is needless to attempt any account of this apparatus, as we could not make it intelligible without figures.

On Thursday the 18th of March a paper was read, by Sir Everard Home, Bart. on the formation of fat by the lower intestines. During the investigations respecting the digestive organs of animals, in which Sir Everard Home has been engaged for many years, he was gradually led to the opinion that after the chyle has been separated from the food in the smaller intestines, it undergoes a new process in the larger intestines, where animal fat is separated from it, and the residue is converted into excrementitious matter. He conceives that the situation of the food in the lower intestines is similar to that of bodies which are converted into adipocire in the ground. He gives an example of this change in Shoreditch church-yard, where several bodies were found converted into adipocire, in the course of ten years. They were buried about ten feet from the common sewer, and about two feet below it. There is always a current of water in this sewer, and at new and full moon it rises two feet higher than usual, and at that time there is water in the graves. Ambergrease is found in the lower intestines of whales: he conceives it to be formed in consequence of a disease in the animal, which prevents the fat from being absorbed as it is formed. Fat is sometimes formed, and thrown out of the human intestines. Of this Sir Everard related several curious cases. He conceives that the formation of fat is owing to the action of the bile on the food in the greater intestines. Mr. Brande, on trying the experiment, found that animal muscle, digested in bile of the temperature 100° for four days, acquired the smell of excrement, and was partly changed into fat. He found that the fæces of fowls, retained in the cœcum for seven days, were converted into fat by the action of diluted nitric acid; but not the fæces in the colon. He found that human fæces, retained in the intestines for six days, contained fat which was separated by digestion in hot water. He related an instance of a child that never grew, though it took food, and was not emaciated; and which, on dissection, was found without a gall bladder or biliary ducts. Hence he conceives that fat was not secreted, and that fat is necessary to the growth of animals. This curious theory throws a new light upon digestion, and explains several circumstances which appeared before anomalous; as the fatty concretions in the gall bladder, which are owing to the action of the bile on the mucus of the bladder.

LINNÆAN SOCIETY.

On Tuesday, March 2d, a paper by Mr. Brown, Librarian to the Society, was read, on some peculiarities in the structure of seeds. All seeds have hitherto been considered as inclosed in a covering; but in some, after they have come to maturity, such covering cannot by any means be separated from the seed. Such seeds have been called *naked* seeds, and they have been divided into two species. Mr. Brown has observed two examples of seeds, not hitherto suspected to exist, namely, seeds absolutely destitute of a covering, from their first appearance till their state of ripeness.

On Tuesday the 16th of March a number of specimens of rotten wood were exhibited by Mr. Sowerby, in order to illustrate the nature of the dry rot in wood, which is occasioned by different species of fungi.

Part of a paper, by the Rev. Patrick Keith, was read, on the cotyledons of grasses. The seeds of grasses, if we do not reckon their outer coats, consist of three parts: 1. A farinaceous substance, which constitutes the greatest part of the seed, and is known by the name of *albumen*. 2. A scale lying upon the surface of the albumen, to which Gærtner has given the name of *vitellus*. 3. The *embryo*, with a kind of sheathy covering. Gærtner considered this sheathy covering as the cotyledon of the grasses. But Dr. Smith conceives the vitellus of Gærtner to be the cotyledon. The object of Mr. Keith's paper is to show that Gærtner's opinion is correct, and that the vitellus does not possess the characters of a cotyledon.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the French Institute for 1812.

(*Continued from* N° III. p. 238.)

WORKS PRINTED.

" *Analytical Theory of Probabilities. By M. le Comte* LAPLACE.

"In our history for 1811 we announced the speedy publication of this work; the two parts of which appeared, after an interval of some months from each other, during the course of 1812. The first part contains the preliminary researches, which were indispensable for understanding the second, in which the author has proposed problems so difficult as to require recourse to a peculiar mode of analysis. After a short notice of the authors who laid the first foundation of the science of probabilities, and of those who made some progress in constructing the edifice, M. le Comte Laplace explains his theory of functions, which he calls *generating one or two variables*. He explains the interpolation of it, the integration, and the transformations. The differentials of these problems contain factors raised to great powers, which require particular rules of integration. It is impossible for us to give any idea of his method here; we cannot even make use of the arranged analysis which occurs at the end of the work. We shall only notice a general remark which terminates the first part. Sometimes the series converge rapidly in their first terms; but this convergence often diminishes, or even changes into divergence. This ought not to prevent us from making use of these series with confidence, employing only the first terms, when the rest of the series which we neglect is the developement of an algebraic function or integral, very small in comparison of that which precedes.

" The second part commences with the general principles, with the definitions of the probability of simple or double events, of the probability of a future event drawn from an event observed, or of an event composed of several others, the respective possibilities of which are given. Numerous applications serve to fix the ideas in a matter so fugitive and so abstruse. We see, then, the laws of probability which result from the indefinite multiplication of events, the probability of errors from the mean results of a great number of observations, and the mean results that are most advantageous. This chapter is one of those of which the applications are the most frequent and the most easy. Philosophers, especially astronomers, may almost make continual use of it. They will find what is the probability that the sum of errors shall be included between such and such a limit. This is the most ordinary case in astronomy. Though it be almost certain that each observation is affected with an error, we know almost always that this error cannot pass a very narrow limit. To correct the tables, we compare them with a great number of observations, each of which gives a relation between the effects of the errors of each of the elements of the tables. M. Laplace determines by his analysis the methods which should lead to the most probable results. He considers the case when there are only two elements to correct, and those in which there are any number whatever; and he always arrives at that method which M. Legendre, who is the first known author of it, calls the method of smaller squares. We must always suppose that the number of observations is very great. It was according to this theory that the tables of M. Burckhardt were judged superior to those of M. Burg, which already possessed so great a degree of precision.

"The same principles apply to the investigation of phenomena, and their causes; and what is very remarkable, we may ascertain the very small effect of a cause always constant, by means of a long series of observations, the errors of which may exceed the effect itself. Thus we may ascertain that the diurnal variation of the barometer depends entirely upon the sun, though these heights are also affected by other inequalities which have not a constant period. We may ascertain the small deviation to the east which the rotation of the earth produces in a body that falls freely from a considerable height. This remark explains how astronomers have been able to determine certain inequalities in the motion of the moon. It was thus that M. Laplace himself was led (knowing the cause) to discover in the motion of the moon two very small inequalities, which depend upon the flatness of the earth, and which they are capable of determining. From the astronomical researches of M. M. Burg and Burckhardt, M. Laplace fixes the flatness at $\frac{1}{304}$. The degrees of France and Peru, of France and the polar circle, have given it $\frac{1}{308}$ to $\frac{1}{309}$. It was likewise by the same method that Laplace was led to his beautiful discoveries respecting the inequalities of Jupiter and Saturn. He concludes from this, "that we ought to be attentive to the indications of nature, when they are the result of a great number of observations, though they should be inexplicable by the methods known. We are so far from knowing all the agents of nature, that it would be unphilosophical to deny the existence of phenomena solely because they are inexplicable in the actual tate of our knowledge. We ought only to examine them with an attention so much the more scrupulous, that it appears more difficult to admit them. The same analysis may be extended to the different results of medicine and political economy, and even to the influence of moral causes; for the action of these causes, when it is repeated a great number of times, offers in its results as much regularity as physical results."

"From these objects, which are particularly interesting to philosophers, the author passes to objects of a more general interest, such as the problems of births. For example, he has found that at Paris the proportion of males born to females is as 25 to to 24; at London as 19 to 18; and in the kingdom of Naples, not including Sicily, as 22 to 21. These ratios approach equality, especially at Paris; but in the three cities the number of males exceeds; and this result appears general, at least in Europe.

"The births furnish one of the most simple and most proper methods to determine the population of a great empire. It gives for France a population of 42,529,267 individuals; and the probability that the error of this statement does not exceed half a million, is as 1161 to 1; that is to say, that we may wager 1161 to 1 that the number is not below 42 millions, nor above 43.

"The same formulæ serve to calculate tables of mortality, to show for every age the number of years of life which may be expected; the mean duration of marriages, or in general of associations between two or more persons; finally, moral expectations.

"Among the different objects which are treated in this work, we have chosen those which are likely to excite the curiosity of the greatest number of readers. Mathematicians will have no choice to make: they will every where find skilful methods united with observations the most ingenious.

"*Of the Defence of Strong Places; a work composed by order of his Imperial and Royal Majesty, for the instruction of the eleves of the Corp du Genie. By* M. CARNOT. *3d Edition.*

"We have formerly announced the first two editions of this important work. The third is distinguished by a preliminary discourse, in which the author shows the necessity of abandoning an imperfect system in order to adopt another, which the progress of the art of attack has rendered necessary; and by two chapters of great interest. One, which is the fourth of the second part, is partly composed of an additional memoir inserted in the second edition, but which has received additions and developements that make it a new work: the other, which is the fifth, presents the respective series of operations of attack and defence, compared together from the commencement of the siege to the end.

"It belongs to skilful military men to judge of this distinguished performance. What M. de Toulongeon, Member of the Institute, said of it a few days before his death, may be seen in the Moniteur; and likewise what M. Ch. Dupin, an officer of the corps of *Genie Maritime*, distinguished for his mathematical knowledge, wrote concerning it a few days later. Eleven beautiful plates add to the value of this new edition.

"*Memoires de Mathematique, concerning Navigation, Physical Astronomy, History. By* M. Bossut.

"The first three memoirs of this volume either obtained or shared the prizes proposed by the Academy of Sciences; and though they have been printed in the collections of that body, it would be difficult at present to procure them. This induced M. Bossut to reprint them. He has added notes, and illustrations of several parts of his History of Mathematics. In the notice that we have taken of this history, in a preceding year, we gave the reasons assigned by M. Bossut in his preface for some involuntary omissions. It appears from the advertisement at the commencement of his new book that these precautions have not had all the success which he had expected; but the proof that his protestations were sincere, appears from the care which he has taken to profit by the notes, and other pieces of information which have reached him. The volume is terminated by the discourse on the life and writings of Paschal, which M. Bossut prefixed to the works of that great writer.

"*Elements of Geometry, with notes. By* M. Legendre. *New Edition.—Descriptive Geometry. By* M. M. Monge *and* Hachette. 3d *Edition.—Theory of Curves of the Second Degree. By* M. Biot. 5*th Edition.—Physics of Fischer, translated from the German, with notes. By* M. Biot. 2d *Edition.*

"No particulars are necessary respecting works whose reputation is fixed. As they have been read and meditated by all who have bought them, we may judge of the good which they have done by the number of editions published in a little time. M. Legendre has inserted in his work the theorems of M. Cauchy respecting polyhedrons.

"M. Humboldt has published the fifth part of his *Views of the Cordelieras*, and of his *Monuments of the Indigenous Inhabitants of America*. We wait with impatience for the first part of his *Historic Journal*, which is in the press.

"The first part of the *Analytical Lectures on Mechanics, delivered at the Imperial Polytechnic School* by M. de Prony, which we announced two years ago, from a copy which the author had commissioned us to present in his name to the Class, was only published during the course of the year 1812, on the return of M. de Prony, retained long in Italy by very interesting labours.

"Among the works sent by the correspondents of the Class, we may notice *two Danish Charts*, transmitted by M. Bugge, Astronomer Royal at Copenhagen: a memoir on the *Construction of Iron Bridges*, by M. Wiebeking: a complete treatise on the *Theory and Practice of Levelling*, by M. Fabre, chief engineer *des ponts et chaussées*.

"Accounts of the works presented to the Imperial Institute by philosophers not members have, as usual, occupied a part of the meetings of the Class, and of the time of those members to whom the examination of them was committed. As we shall notice here only the objects which have received the fullest approbation, we shall confine ourselves to the memoirs, of which the following are the titles:—

"*Memoir* of M. Cauchy, *on Polygons and Polyhedrons.* M. Legendre has inserted the results into his Elements.

"*Plain and Object Glasses*, by M. le Rebours. In consequence of this Report, the Board of Longitude obtained for the Imperial Observatory a telescope of M. le Rebours, which appears the best that exists of the same dimensions.

"*Memoir on radiant Heat*, by M. François de la Roche.

"*Memoir* of M. Binet, jun. *on the Calculation of the Planetary Disturbances.*

"*New Hydraulic Machine*, by M. Lingois.

"*Experiments* of M. Jecker *in Optics, Navigation, and Astronomy.*

"*Memoir* of M. Gauthier *on the general methods of constructing graphically a circle determined by three conditions, and a Sphere determined by four.*

"*Memoir* of M. Servois, *to destroy the Differential Calculus of the Calculus of Differences.*

"*New Stocking Loom*, by M. Favreau. On occasion of this memoir, M. Desmarets, the reporter, collected the improvements which other artists had formerly introduced into this machine. He composed a paper on the subject, which the Class judged useful to the history of the art, and which it ordered to be printed in a succeeding volume.

"At the last meeting of the year the Class heard two very interesting reports. One on a manuscript work, entitled *Developements of Rational and Analytical Geometry, containing the theory of the Curvatures of Surfaces, with applications to the Stability of Vessels, to Loading and Unloading; and to Optics*, by M. Dupin, *Capitaine du Corps de Genie Militaire.* This manuscript constitutes part of a more considerable work, which the author means to submit to the judgment of the Class, and which is the fruit of the few moments of leisure afforded him in a very active service, with continual change of place. The other memoir had for its object various machines by means of which M. Mannoury Dectot has resolved, in a manner as varied as ingenious, this hydraulic problem, which has the air of a paradox: *To raise water by means of machines, of which all the parts are constantly immoveable, and which of course are neither furnished with piston, nor valve, nor any thing equivalent.*" *

(*To be continued.*)

Article XI.

New Patents.

Robert Dickinson, of Great Queen-street, Lincoln's Inn Fields, in the county of Middlesex, Esq. and Henry Maudsley, in the parish of St. Mary Lambeth, in the county of Surrey, engineer; for a process for sweetening water and other liquids, and applicable to other purposes. Dated February 8, 1812.

The process recommended is to blow a current of air through the water. There is nothing new in this process. It was publicly recommended long ago, and tried in the British navy. We believe it was proposed by Dr. Hailes.

William Chapman, of Murton House, in the county of Durham, civil engineer, and Edward Walton Chapman, of Wellington Ropery, in the parish of Wallsend, in the county of Northumberland, rope-maker; for a method or methods of facilitating the means, and reducing the expense, of carriage on railways, and other roads. Dated December 30, 1812.

Joseph Raynor, of Sheffield, in the county of York, cotton-spinner; for improved machinery for roving and spinning cotton, silk, flax, and wool. Dated January 1, 1813.

William Wilkinson, of Grimesthorpe, in the county of York, shear-smith; for improved horse shears, wool shears, and glovers' shears. Dated January 5, 1813.

William Allen, of the Curtain-road, Shoreditch, for an improvement on machinery to be worked by wind. Dated January 15, 1813.

William Bundy, of Camden-town, in the county of Middlesex, mathematical instrument maker; for an improvement in the manufacture of lint. Dated January 15, 1813.

Matthew Bush, of Longford, in the county of Middlesex, calico printer; for improvements for printing calicoes. Dated January 15, 1813.

Richard Cawkwell, of Newark-upon-Trent, in the county of Nottingham, miller; for a machine for washing, cleansing,

* An account of these curious machines will be found in the third number of this Journal.

and scowering linen and woollen goods, and other articles. Dated January 15, 1813.

Robert Dickinson, of Great Queen-street, Lincoln's Inn Fields, in the county of Middlesex, Esq.; for an improvement in vessels for containing liquids. Dated January 15, 1813.

John Shorter Morris, of North Market-street, Kennington, in the county of Surrey, mechanic; for a machine or engine upon a new or superior principle, which contains a new way for a man or men to use his or their power and strength, to be used as a crane, or to give a rotatory motion to any machine, engine, or mill work. Dated January 15, 1813.

Thomas Ryland, of Birmingham, in the county of Warwick, plater; for a fender for fire-places. Dated January 15, 1813.

Charles Gröll, of Leicester-place, Leicester-square, in the county of Middlesex, and Frederick Dizi, of Park-place, Baker-street North, in the said county of Middlesex; for certain improvements on harps. Dated January 22, 1813.

Marc Isambard Brunel, of Chelsea, in the county of Middlesex, civil engineer; for certain improvements in saw mills. Dated January 26, 1813.

Francis Crow, of Feversham, in the county of Kent, watch-maker and silversmith; for improvements in the mariner's compass, or boat compass. Dated January 30, 1813.

Robert Dunkin, of Penzance, in the county of Cornwall; for methods of lessening the consumption of steam and fuel in working fire-engines; and also methods for the improvement of certain instruments useful for mining, or other purposes. Dated January 30, 1813.

William Broughton, of Rose-court, Tower-street, in the city of London, joiner; for a method of making a peculiar species of canvas, which may be used more advantageously for military and other purposes. Dated February 4, 1813.

George Alexander, watch-maker, in Leith; for a mode of suspending the card of the mariner's compass, being on a principle entirely new. Dated February 4, 1813.

Joseph Hamilton, of the city of Dublin, Gent.; for certain new methods of constructing earthen building materials. Dated February 20, 1813.

John Roberts, of Macclesfield, in the county of Chester, cotton-spinner; for a method of concentrating or reducing into small compass, such parts of the malt and hops as are requisite in making ale, beer, and porter. Dated February 20, 1813.

Charles Plimley, of Birmingham, in the county of Warwick, manufacturer; for means or methods of working steel or iron, or steel joined with iron, in or into taper forms, whether round or square, or of any other figure, in the cross sections thereof, for the purpose of making files, and various other articles. Dated February 20, 1813.

Joseph Smith, of Cosely, in the parish of Sedgley, in the county of Stafford, iron and coal-master; for certain improvements in the construction and manufacture of iron and other chains, whereby a considerable expense will be saved in the making thereof, and the same rendered more durable. Dated February 24, 1813.

Article XII.

Scientific Books in hand, or in the Press.

Elford Leach, Esq. is about to publish Elements of Zoology, with tabular views of the genera, after the manner of Dumenil.

The third volume of the Memoires d'Arcueil is in the press.

Professor Stewart has in the press a second volume, in 4to. of Elements of the Philosophy of the Human Mind.

A new volume of the Transactions of the Literary and Philosophical Society of Manchester is nearly ready for publication.

A Translation of the Travels of Leopold Von Buch in Norway and Lapland has been undertaken by Mr. Black.

Dr. Thomson's Travels in Sweden will certainly be ready by the 1st of May.

Dr. Bancroft is printing, in two volumes 8vo. a new and enlarged edition of Experimental Researches concerning the Philosophy of Permanent Colours.

A Supplement to Montague's Ornithological Dictionary is in preparation.

The Rev. W. Gunn is printing, in one volume, an Inquiry into the Origin and Influence of Gothic Architecture, illustrated by plates.

*** *Early Communications for this Department of our Journal will be thankfully received.*

Article XIII.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Barometer. Min.	Barometer. Med.	Thermometer. Max.	Thermometer. Min.	Thermometer. Med.	Evap.	Rain.	
2d Mo.										
Feb. 22	S W	29·80	29·69	29·745	57	41	49·0	—		☽
23	N W	30·03	29·80	29·915	45	35	40·0	—		
24	W	30·14	30·03	30·085	49	32	40·5	—	—	
25	S W	30·13	29·83	29·980	50	35	42·5	—	0·71	
26	S W	29·90	29·70	29·800	52	35	43·5	·50	8	
27	N W	30·33	29·90	30·115	46	32	39·0	—		
28	N W	30·36	30·30	30·330	50	34	42·0	·23		
3d Mo.										
March 1	S W	30·30	30·20	30·250	51	39	45·0	—		
2	S W	30·20	30·02	30·110	47	35	41·0	—	—	●
3	Var.	30·33	30·02	30·175	52	32	43·0	—	—	
4	S W	30·33	30·20	30·265	51	36	43·5	—	6	
5	W	30·34	30·20	30·270						
6	W	30·40	30·34	30·370	53	35	44·0	—		
7	N W	30·40	30·30	30·350	49	39	44·0	—		
8	N W	30·30	30·20	30·250	54	43	48·5	—		
9	N W	30·20	29·89	30·045	52	36	44·0	·56	—	☾
10	E	29·96	29·89	29·925	42	26	34·0	—	0·12	
11	N E	30·21	29·96	30·085	39	24	31·5	—	—	
12	N E	30·27	30·21	30·240	37	24	30·5	—		
13	N W	30·27	30·20	30·235	40	29	34·5	—		
14	S W	30·20	30·10	30·150	47	40	43·5	·19		
15	S W	30·18	30·10	30·140	53	43	48·0	—		
16	Var.	30·18	30·09	30·135	51	32	41·5	—		
17	N E	30·09	29·96	30·025	56	32	44·0	—		○
18	N W	29·96	29·96	29·960	58	36	47·0	—		
19	E	29·96	29·78	29·870	58	40	49·0	—		
20	S W	29·96	29·78	29·870	56	35	45·5	—	0·14	
21	S W	29·96	29·84	29·900	53	42	47·5	·31		
22	S W	30·25	29·96	30·105	55	33	44·0	—		
23	N W	30·30	30·28	30·170	50	34	42·0	—		
24	W	30·28	29·98	30·130	47	39	43·0	·18	0·35	
		30·40	29·69	30·109	58	24	42·50	1·97	1·46	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Second Month. 24. Hoar frost. About 6 p. m. a very dark cloud came over, lowering with an arched base, as before thunder, and presently discharged a shower of large hail and rain, which was accompanied with a cold wind. 25. Fair a. m.; wet and windy p. m. and night. 26. The same. 27. *Cirrus*, *Cumulus*, and *Cirrostratus* colours, appeared together: much wind: about 7 p. m. Wind N.W. a bright meteor passed from about the zenith towards the N. declining a little westward. 28. Clear morning: wind moderate.

Third Month. 1. Hoar frost: fair. 2, 3. Light showers. A *Nimbus* appeared S. of the setting sun on the 3d, which went away southward. 9. Light showers. 10. a. m. Sleet. At sunset, a *Cumulostratus*: some hail-balls in the night. 11. A *Nimbus* was perceptible by 7 a. m. forming in the N. E. Some heavy (though transient) snow; squalls followed during the day. Abundance of snow fell, on this and the following night, to the southward, extending as far as the coast of France.

RESULTS.

Prevailing Winds Westerly; with a marked interruption by a current from the N. E. occasioning snow about the middle of the period.

Barometer : greatest observed height ..30·40 inches;
Least..................29·69 inches;
Mean of the period....30·109 inches.

Thermometer: greatest height ....58°
Least ....24°
Mean of the period ....42·50°

Evaporation, 1·97 inches. Rain, &c. 1·46 inches.

Tottenham, L. HOWARD.
Third Month, 25, 1813.

Errata in the last Number.

Page 162, line 9 from bottom, *for* "Red Noble Order of the Third Class," *read* "Of the Third Class of the Order of the Red Eagle."

Page 192, line 10 from bottom, *for* "*of* a new set of rocks, or a new arrangement, *if* those already known," *read* "*if* a new set of rocks, or a new arrangement *of* those already known."

Same page, line 14 from bottom, *for* "that which leads," *read* "that charm which leads."

ANNALS

OF

PHILOSOPHY.

MAY, 1813.

Article I.

Biographical Account of M. de Fourcroy. By Thomas Thomson, M.D. F.R.S.

LITERARY men may be divided into three classes. Some make a great figure during their life-time; but death erases their names from the annals of science, and they sink into the grave and obscurity at once. Such were Dr. Mead and Sir John Hill. Some are little known during their life-time, and spend their days in obscurity and penury; but when death has once closed the scene, their reputation rises untarnished by envy, and unsullied by emulation, and flows on like a mighty river, the broader, and deeper, and greater, the farther it advances. Such, in some respects, were Kepler and Scheele. Some are so unfortunate, through imprudence, or a perverse train of circumstances, neither to acquire reputation during their lives, nor after their death; while their more fortunate contemporaries, with less labour, and less merit, gather all the laurels which they had earned. It would be invidious to mention the names of any who unfortunately belong to this class; but they will readily occur to every one acquainted with the history of science. Every tyro in algebra is familiar with Cardan's rules for the solution of cubic equations, while the name of the real discoverer of these rules is scarcely known, except to mathematical antiquaries. M. de Fourcroy, the subject of this article, made so conspicuous a figure during his life-time, that it would by no means surprise us if he should finally take his place among that class of literary men whom we characterised in the first place: not that he wanted merit; for it is not so much merit, as a regard to distributive justice, which leads to the classification. Who will be hardy enough to affirm that Churchill wanted merit as a poet? During his short and rapid literary career he appeared to wield the thunderbolts in his hand, and was an object of dread and adoration, like a kind of Divinity. But where is his reputation now? It has sunk, since his death, as much below the true level, as it rose above it during his life-time. And this we believe will always be the case. Mankind will atone for the excessive adulation which they pay to a man during his life-time, by a corresponding negligence after his death.

Antoine François de Fourcroy, Comte of the French Empire, Councellor of State, Commander of the Legion of Honour, Member of the Institute, and of most scientific societies in Europê, Professor of Chemistry at the Museum of Natural History, Professor of the Faculty of Medicine at Paris, and Teacher in the Polytechnic School, was born at Paris, on the 15th of June, 1755, and was the son of Jean Michel de Fourcroy and of Jeanne Laugier.

His family had long resided in the capital, and several of his ancestors had distinguished themselves at the bar. One of them, during the reign of Charles IX. was honoured with the epithet of *fori decus*.

Antoine François de Fourcroy sprung from a branch of the family that had gradually sunk into poverty. His father exercised in Paris the trade of an apothecary, in consequence of a charge which he held in the house of the Duke of Orleans. The Corporation of Apothecaries having obtained the general suppression of all such charges, M. de Fourcroy, the father, was obliged to renounce his mode of livelihood; and his son grew up in the midst of the poverty produced by the monopoly of the privileged bodies in Paris. He felt this situation the more keenly, because he possessed from nature an extreme sensibility of temper. When he lost his mother, at the age of seven years, he attempted to throw himself into her grave. The care of an elder sister preserved him with difficulty till he reached the age at which it was usual to be sent to the college. Here he was unlucky enough to meet with a brutal master, who conceived an aversion to him, and treated him with cruelty. The consequence was a dislike to study; and he quitted the college at the age of 14, somewhat less informed than when he went to it.

His poverty now was such, that he was under the necessity of endeavouring to support himself by commencing writing-master. He had even some thoughts of going upon the stage; but was prevented by the hisses bestowed upon a friend of his, who had unadvisedly entered upon that perilous career, and was treated in consequence without mercy by the audience. While uncertain what plan to follow, the advice of Viq. d'Azyr induced him to commence the study of medicine.

This great anatomist was an acquaintance of M. de Fourcroy, the father. Struck with the appearance of his son, and the courage with which he struggled with his bad fortune, he conceived an affection for him, and promised to direct his studies, and even to assist him during their progress. The study of medicine to a man in his situation was by no means an easy task. He was obliged to lodge in a garret, so low in the roof that he could only stand upright in the centre of the room. Beside him lodged a water-carrier, with a family of 12 children. Fourcroy acted as physician to this numerous family; and in recompense was always supplied with abundance of water. He contrived to support himself by giving lessons to other students, by facilitating the researches of richer writers, and by some translations which he sold to a bookseller. For these he was only half paid; but the conscientious bookseller offered, 30 years afterwards, to make up the deficiency, when his creditor was become Director General of Public Instruction.

Fourcroy studied with so much zeal and ardour that he soon became well acquainted with the subject of medicine. But this was not sufficient. It was necessary to get a Doctor's degree; and all the expenses, at that time, amounted to 250*l.* sterling. An old physician, Dr. Diest, had left funds to the faculty to give a gratuitous degree and license, once every two years, to the poor student who should best deserve them. Fourcroy was the most conspicuous student at that time in Paris. He would therefore have reaped the benefit of this benevolent institution, had it not been for the unlucky situation in which he was placed. There happened to exist a quarrel between the faculty charged with the education of medical men and the granting of degrees, and a society recently established by Government for the improvement of the medical art. This dispute had been carried to a great length, and had attracted the attention of all the frivolous and idle inhabitants of Paris. Viq. d'Azyr was secretary to the society, and of course one of its most active champions, and was in consequence particularly obnoxious to the faculty of medicine at Paris. Fourcroy was unluckily the acknowledged protegée of this eminent anatomist. This was sufficient to induce the faculty of medicine to refuse him a gratuitous degree. He would have been excluded in consequence from entering upon the career of a practitioner, had not the society, enraged at this treatment, and influenced by a violent party spirit, formed a subscription, and contributed the necessary expenses.

It was no longer possible to refuse M. de Fourcroy the degree of Doctor, when he was thus enabled to pay for it. But above the simple degree of Doctor, there was a higher one, entitled, *Docteur Regent*, which depended entirely upon the votes of the faculty. It was unanimously refused to M. de Fourcroy. This refusal put it out of his power afterwards to commence teacher in the medical school, and gave the medical faculty the melancholy satisfaction of not being able to enrol among their number the most celebrated professor in Paris. This violent and unjust conduct of the faculty of medicine made a deep impression in the mind of Fourcroy, and contributed not a little to the subsequent downfall of that powerful body.

Fourcroy being thus entitled to practise in Paris, his success depended entirely upon the reputation which he could contrive to establish. For this purpose he devoted himself to the sciences connected with medicine, as the shortest and most certain road by which he could reach his object. His first writings showed no predilection for any particular branch of science. He wrote upon chemistry, anatomy, and on natural history. He published an *Abridgment of the History of Insects*, and a *Description of the Bursæ Mucosæ of the Tendons*. This last piece seems to have given him the greatest celebrity: for in 1785 he was admitted, in consequence of it, into the Academy of Sciences as an anatomist; but the reputation of Bucquet, which at that time was very high, gradually directed his particular attention to chemistry, and he retained this predilection during the rest of his life.

Bucquet was at that time professor of chemistry in the medical school of Paris, and was then greatly celebrated and followed, on account of his eloquence and the elegance of his language. Fourcroy became in the first place his pupil, and soon after his particular friend. One day, when an unforeseen disease prevented him from lecturing as usual, he entreated M. de Fourcroy to supply his place. The young philosopher at first declined, and alleged his total ignorance of the method of addressing a popular audience. But, overcome by the persuasions of Bucquet, he at last consented; and in this his first essay, he spoke two hours without disorder or hesitation, and acquitted himself to the satisfaction of his whole audience. Bucquet soon after substituted him in his place, and it was in his laboratory and in his class-room that he first made himself acquainted with chemistry. He was enabled at the death of Bucquet, in consequence of an advantageous marriage which he had made, to purchase the apparatus and cabinet of his master; and although the Faculty of Medicine would not allow him to succeed to the chair of Bucquet, they could not prevent him from succeeding to his reputation.

There was a kind of college established in the King's Garden, which was at that time under the superintendance of Buffon, and Macquer was the professor of chemistry in this institution. On the death of this chemist, in 1784, Lavoisier stood candidate for the chair. But Buffon received more than a hundred letters in favour of Fourcroy; and the voice of the public was so loud in his favour, that he was appointed to the situation, in spite of the high reputation of his antagonist, and the superior interest that might be supposed to result from his fortune and his situation.

Fourcroy continued professor at the Jardin des Plantes during the remainder of his life, which lasted 25 years; and such was his eloquence, or so well was it fitted to the taste of the French nation, that his celebrity as a lecturer continued always upon the increase: so great also were the crowds, both of men and women, that flocked to hear him, that it was twice necessary to enlarge the size of the lecture-room. I had myself an opportunity of hearing him lecture two or three times, and must acknowledge that I found it difficult to account for the celebrity which he enjoyed. His style was precisely similar to that of his books, flowing and harmonious, but very diffuse, and destitute of precision; and his manner was that of a petit maitre, mixed with a good deal of pomposity, and an affectation of profundity. There must be something, however, in such a manner, capable of attracting the generality of mankind; for I know a professor who possesses as much of it as is consistent with the British character, and who is far inferior to Fourcroy as a man of science; who, nevertheless, enjoys within his own sphere nearly the same degree of popularity that Fourcroy did in his.

We must now notice the political career which Fourcroy ran, during the progress of the revolution. In a country where political changes were going on with so much rapidity, and where every description of men were successively had recourse to, it was not possible that a professor so much admired for his eloquence could escape observation. Accordingly, he was elected a member of the National Convention in the autumn of 1793. The National Convention, and France herself, were at that time in a state of abject slavery; and so sanguinary was the tyrant who ruled over that unhappy country, that it was almost equally dangerous for the members of the Convention to remain silent, or to take an active part in the business of that assembly. Fourcroy, notwithstanding his reputation for eloquence, and the love of eclat which appears all along to have been his domineering passion, had good sense enough to resist the temptation, and never opened his mouth in the Convention till after the death of Robespierre. This is the more to be wondered at, and is a greater proof of prudence, as it is well known that he took a keen part in favour of the revolution, and that he was a determined enemy to the old order of things, from which he had suffered so severely at his entrance into life.

At this period he had influence enough to save the life of some men of merit; among others, of Darcet, who did not know the obligation he lay under to him till long after. At last his own life was threatened, and his influence of course utterly annihilated.

During this unfortunate and disgraceful period, several of the most eminent literary characters of France were destroyed; among others, Lavoisier; and Fourcroy has been accused of contributing to the death of this illustrious philosopher, his former rival, and his master in chemistry. How far such an accusation is deserving of credit, I for my part have no means of determining; but Cuvier, who was upon the spot, and in a situation which enabled him to investigate its truth or falsehood, acquits Fourcroy entirely of the charge, and declares that it was urged against him merely out of envy at his subsequent elevation. "If in the rigorous researches which we have made," says Cuvier, in his Eloge of Fourcroy, "we had found the smallest proof of an atrocity so horrible, no human power could have induced us to sully our mouths with his Eloge, or to have pronounced it within the walls of this temple, which ought to be no less sacred to honour than to genius."

Fourcroy began to acquire influence only after the 9th thermidor, when the nation was wearied with destruction, and when efforts where making to restore those monuments of science, and those public institutions for education, which, during the wantonness and folly of the revolution, had been overturned and destroyed. Fourcroy was particularly active in this renovation, and it was to him chiefly that almost all the schools established in France for the education of youth are to be ascribed. The Convention had destroyed all the colleges, and universities, and academies, throughout France. The effects of this ridiculous abolition soon became visible. The army stood in need of surgeons and physicians, and there were none educated to supply the vacant places. Three new schools were founded for educating medical men. They were nobly endowed, and still continue connected with the University of Paris. The term *schools of medicine* was proscribed as too aristocratical. They were distinguished by the ridiculous appellation of *schools of health*. The *Polytechnic School* was next instituted, as a kind of preparation for the exercise of the military profession, where young men could be instructed in mathematics and natural philosophy, to make them fit for entering the schools of the artillery, of genius, and of the marine. The central schools was another institution for which France is indebted to the efforts of Fourcroy. The idea was good, though it has been very imperfectly put in execution. It was to establish a kind of university in every department, for which the young men were to be prepared by means of a sufficient number of inferior schools scattered through the department. But these inferior schools have never been either properly established or endowed; and even the central schools themselves have never been supplied with proper masters. Indeed it would have been impossible to have furnished such a number of masters at once. On that account an institution was established at Paris, under the name of *Normal School*, for the express purpose of educating a sufficient number of masters to supply the different central schools.

Fourcroy, either as member of the *Convention*, or of the *Council of Ancients*, took an active part in all these institutions, both as far as regarded the plan and the establishment. He was equally concerned in the establishment of the Institute, and of the *Museum d'Histoire Naturelle*. This last was endowed with the utmost liberality, and Fourcroy was one of the first professors; as he was, also, in the School of Medicine, and the Polytechnic School. He was equally concerned in the restoration of the University, which constitutes the most splendid part of Bonaparte's reign, and the part which will be longest remembered with gratitude and applause.

The violent exertions which M. de Fourcroy made in the numerous situations which he filled, and the prodigious activity which he displayed, gradually undermined his constitution. He himself was sensible of his approaching death, and announced it to his friends as an event which would speedily take place. On the 16th of December, 1809, after signing some dispatches, he suddenly cried out, *Je suis mort*, and dropt lifeless on the ground.

He was twice married: first to Mademoiselle Bettinger, by whom he had two children; a son, an officer in the artillery, who inherits his title; and a daughter, Madame Foucaud. He was married a second time to Madame Belleville, the widow of Vailly, by whom he had no family. He left but little fortune behind him; and two maiden sisters who lived with him, depended, for their support, upon his friend M. Vauquelin.

The character of M. de Fourcroy is sufficiently obvious. It was exactly fitted to the country in which he lived, and the revolutionary government, in the midst of which he was destined to finish his career. Vanity was his ruling passion, and the master spring of all his actions. It was the source of all the happiness, and of all the misery of his life; for every attack, from what quarter soever it proceeded, was felt by him with equal acuteness. The sneer of the most ignorant pretender, or the most obscure paper, affected him just as much as if it had proceeded from the most profound philosopher. It is needless to observe, after this, how much he must have suffered from the various parties into which the French chemists divided themselves: all of which were more or less hostile to him, excepting the one which he himself headed. His occupations were too numerous, and his elocution too ready, to put it in his power either to make profound discoveries, or to compose treatises of great depth or originality. The changes which took place in the science of chemistry were brought about by others, who were placed in a different situation, and endowed with different talents; but no man contributed so much as Fourcroy to the popularity of the Lavoisierian opinions, and the rapidity with which they were propagated over France, and most countries in Europe. His eloquence drew crowds to hear him, and persuaded his audience to embrace his opinions.

He must have possessed an uncommon facility in writing, for his literary labours are exceedingly numerous. Besides those essays which have been already noticed, he published five editions of his System of Chemistry, each of them gradually increasing in size and value; the first edition being in two volumes, and the fifth in ten. This last edition he wrote in 16 months. It contains a vast quantity of valuable matter, and contributed considerably to the general diffusion of chemical knowledge. Its fault is the diffuseness of the style, and the want of correct references. The readers of Fourcroy's system would suppose that all the discoveries in chemistry have been made by the French, and that other nations have contributed comparatively little to the stock of chemical knowledge; whereas, in reality, the very opposite is the truth. A much greater number of important chemical discoveries have been made in Britain than in France; and the British chemists have contributed prodigiously to the raising of that beautiful fabric which we at present admire.

Perhaps the best of all Fourcroy's productions is his *Philosophy of Chemistry*, which is remarkable for its conciseness, its perspicuity, and the neatness of its arrangement.

Besides these works, and the periodical work called *Le Medicin Eclairé*, of which he was the editor, there are above 160 papers on chemical subjects, with his name attached to them as the author, which appeared in the Memoirs of the Academy, of the Institute, in the Annales de Chimie, or the Annales de Museum d'Histoire Naturelle, of which last work he was the original projector. As in most of these papers the name of Vauquelin is associated with his own, as the author; and as during the publication of those which appeared with his own name alone, Vauquelin was the operator in his laboratory, it is not possible to determine what part of the experiments were made by Fourcroy, and what by Vauquelin. I have been told, by a gentleman who had a good opportunity of getting information on the subject, that almost all the experiments were made by Vauquelin, but that all the papers were written by Fourcroy himself. The discoveries contained in these numerous dissertations are of considerable importance, and relate chiefly to animal and vegetable chemistry. At the same time, it must be allowed that Fourcroy and Vauquelin often fall into mistakes difficult to be accounted for; and that they do not in every case do justice to their predecessors or contemporaries who had been occupied with the same investigations. A complete enumeration of these dissertations would probably be considered as too tedious. I shall therefore satisfy myself with pointing out some of his most important chemical discoveries and observations.

1. He repeated the curious experiments of Berthollet upon the evolution of azotic gas from animal substances. This dissertation contains very little new, and is remarkable for some striking mistakes; as for example, that azotic gas has the property of giving a green colour to vegetable blues. If such an observation was ever actually made, he must have been deceived by a portion of ammonia mixed with the azotic gas. He announced, soon after, that the air contained in the swimming bladder of the carp is azotic gas. This paper contains several absurd observations on the method of procuring azotic gas: as for example, that the black oxide of manganese gives out pure azote, if exposed to a heat below redness.

2. He analysed a green coloured mineral from Auvergne, which he found a mixture of arseniate of lead and phosphate of lead.

3. He affirmed that ammonia is decomposed by the oxides of manganese, mercury, and iron; and that these oxides, at the same time, lose either the whole or a portion of their oxygen.

4. He ascertained that the most common constituent of biliary calculi is a substance very similar in its properties to spermaceti. This substance, in consequence of a subsequent discovery which he made, during the removal of dead bodies from the burial-ground of the Innocents at Paris, namely, that these bodies were converted into a fatty matter, got the name of adipocire.

5. He found that vegetable juices frequently contain a substance which coagulates when the juice is exposed to a gentle heat. This substance he considered as *albumen*; but Proust afterwards showed that it was, in reality, a species of gluten, and quite different in its properties from albumen.

6. He ascertained the properties of several triple salts, which magnesia, and ammonia, and an acid, are capable of forming; and explained, by this discovery, the reason why magnesia is not precipitated completely from its solutions by ammonia. This paper, which appeared in the fourth volume of the Annales de Chimie, I consider as one of the best ever published by Fourcroy.

7. He published a very elaborate analysis of the quinquina, a

species of bark from St. Domingo, which was considered at the time as a model for vegetable analysis. It bears the exact characters of the peculiar method followed by Vauquelin. I should suppose therefore, though nothing is said on the subject, that the experiments were contrived and executed by that eminent chemist.

8. His dissertation on the sulphate of mercury, though imperfect, contains some good observations, and facilitated the knowledge of metallic salts, which was at that time very imperfect; but has been greatly improved since. The same remark applies to his subsequent memoirs on the action of ammonia, on the sulphate, nitrate, and muriate, of mercury. These papers contain some mistaken opinions, though the formation of the triple salts, which constitutes the basis of his opinion, be correct.

9. His experiments on the brain contains several valuable facts, and his opinion approaches to accuracy. The subject has been recently resumed by Vauquelin, who has published a curious dissertation on it, which we shall insert in the present number of the *Annals of Philosophy*.

10. The analysis of tears, and the mucus of the nose, by Fourcroy and Vauquelin, is valuable; though it contains some mistakes, from the too hasty application of an erroneous theory to the animal phenomena.

11. The analysis of urine, and of urinary calculi, by the same gentlemen, has been much admired on the Continent, and no doubt contains many important facts; but hardly any important addition is made in it to the dissertation of Dr. Wollaston on the same subject, which had been already published in the Philosophical Transactions. To this very important paper no allusion whatever is made; yet they could hardly be ignorant of it, as they quote Dr. Pearson's essay on the same subject, which had been published in the same work.

12. Their experiments on the combustion of bodies in oxymuriatic acid gas, and the detonations which take place when hyperoxymuriate of potash and a combustible substance are mixed together and struck upon an anvil, are curious, though they add but little to the improvement of the theory of chemistry.

13. Their method of obtaining barytes in a state of purity, by exposing the nitrate of barytes to a red heat in a porcelain crucible, is a good one; and is by far the easiest way to procure that earth in a state of tolerable purity.

14. Their theory of the formation of sulphuric ether, by the action of sulphuric acid, is plausible; and at least as likely to be true as any other explanation which has been hitherto offered: but they were wrong in attempting to extend that theory to the

formation of ether in general. We now know that the nitric, and muriatic, and acetic ether, are formed in quite a different manner.

15. They ascertained by experiment that the three liquids, known by the names of pyromucous, pyrolignous, and pyrotartarous acids, are nothing else than vinegar holding in solution a portion of empyreumatic oil.

16. They ascertained the presence of phosphate of magnesia in the bones of all animals.

17. Their experiments upon crude platina were not so successful. They detected in it the presence of a new metal. But as they in fact confounded the two metals of Tennant, the osmium and iridium, together, all their observations were either erroneous, or so confused that it was impossible to disentangle the truth from them.

18. Their experiments on the bitter principle extracted from indigo, and the detonating property which it possesses, are curious. The subject was carried farther by Hatchett and Chevreul.

19. They were unsuccessful in their attempts to detect the presence of fluoric acid in bones; though this was afterwards successfully executed by Berzelius.

20. They discovered a quantity of uncombined phosphorus in the melts of fishes. They showed, likewise, an analogy between the pollen of the antheræ of some flowers, and the seminal fluid of animals.

21. They detected in the common onion the presence of a considerable quantity of saccharine matter, and showed by experiment that this saccharine matter was converted into manna by a spontaneous change which it underwent. They found, at the same time, that manna is incapable of undergoing the vinous fermentation, and, of course, that it does not yield alcohol.

22. They ascertained the properties of animal mucus, and showed that it differed from all other animal substances.

23. These, though only a small number of the chemical papers published by Fourcroy, are by far the most important. We have no means of determining what portion of each belongs to Fourcroy, and what to Vauquelin; but there is one merit, at least, which cannot be refused Fourcroy, and it is no small one. He formed and brought forwards Vauquelin, and proved to him ever afterwards a most steady and indefatigable friend. This is bestowing no small panegyric on his character; for it would have been impossible to have retained such a friend through all the horrors of the French revolution, if his own qualities had not been such as to merit so steady an attachment. I have taken

no notice of the labours of M. de Fourcroy in the chemical part of the Encyclopedie Methodique, though they are rather voluminous, because I conceive them of inferior importance to those which I have noticed.

Article II.

Analysis of the Cerebral Matter of Man, and some other Animals. By M. Vauquelin.*

Sect. I.

History of the chemical labours hitherto undertaken on the cerebral matter.

Although the brain, in consequence of the functions which it is supposed to perform, ought to have early excited the curiosity of chemists, yet one is surprised to find but very little in their works concerning its chemical nature. Even the small number of experiments which have been undertaken have not been pushed far enough to enable us to deduce any positive consequences. Hence the opinions formed respecting the composition of the brain are erroneous, or at least incomplete. It was therefore necessary to resume the subject from the commencement, and to employ that care and precision which the difficulty of the subject rendered necessary. I have undertaken this difficult task. I submit the results which I have obtained to the chemists. It is their province to judge how far I have succeeded.

Gurman first announced the long period during which the brain remains sound in the cranium of dead bodies.

Burrhus compared this organ to an oil, and particularly to spermaceti.

Thouret, whose loss Medicine laments, in an excellent memoir on the dead bodies found in the burying-ground of the Innocents, considered the substance of the brain as a sort of soap.

Fourcroy, whom the sciences likewise deplore, advanced an opinion respecting the nature of the cerebral matter different from that of Thouret.† He considered it as principally composed of albumen and of another matter, which he thought a peculiar substance. Though the experiments of Fourcroy leave several things imperfect, yet it will be seen, by comparing them

* From the Annales de Chimie, vol. lxxxi. p. 37.

† Annales de Chimie, vol. xvi.

with mine, that his account of the brain is by far the completest hitherto given, and that it approaches pretty closely to the truth.

Sect. II.

Treatment of the brain with alcohol, or spirit of wine.

A portion of human brain, deprived of its envelopes, and reduced to a homogeneous pulp in a marble mortar by means of a wooden pestle, was mixed with about five times its weight of alcohol of 36 degrees. This mixture, left to macerate during 24 hours, was heated to the boiling temperature, and passed through the filter.

The alcohol had acquired a greenish colour. It deposited, on cooling, a white matter, partly in flocks, and partly in plates.

Twelve hours after the cooling, the alcohol was filtered again. It still retained its green colour. Water destroyed its transparency, and rendered it milky.

This alcohol, being evaporated till only one eighth part of it remained, deposited, on cooling, an oily matter, yellowish and fluid, which sunk to the bottom of the vessel. The liquid itself continued yellowish.

We shall hereafter examine this oily matter, together with the liquor which accompanied it.

The alcohol obtained by distillation was poured upon the cerebral matter, already once digested with alcohol, as has been already said.

After having boiled the mixture for a quarter of an hour, the alcohol was filtered while hot. It passed through the filter with a colour approaching to blue, and deposited, on cooling, a white matter, as in the first operation, but less abundant. The alcohol, after having deposited this matter, still became milky when mixed with water. This alcohol, when distilled, passed without colour; and the residue of the distillation, which amounted to about the 28th part of the liquid subjected to distillation, had lost its green colour, and acquired a yellow colour.

This residue exhibited two sorts of liquors; one which had the aspect of an oil, and occupied the bottom of the vessel; the other, less coloured, resembled a solution of gum.

We defer the examination of these two liquids till we come to describe those which were obtained by the first operation, because we suspect them to be of the same nature.

The white matter deposited by alcohol in the first operation, and that which the same liquid allowed to deposite in the second operation, had a pasty consistence, a greasy and glutinous feel, a brilliant and satiny appearance.

The last portion was whiter and more solid; but being melted, it was changed, like the first, upon being brought near the flame of a candle.

These substances, when dried upon filtering paper, rendered it transparent, and stained it as an oil would have done.

The matter, which had been retained in solution by the alcohol, and which had been separated by the distillation of this liquid, had a yellow colour, and was of the consistence of a paste, and adhesive. When dried, it dissolved again in boiling alcohol; but before entering into combination with the liquid, it melted at the bottom of the vessel, and assumed the appearance of an oil. The alcoholic solution deposites, on cooling, two matters, which probably differ from each other in the aspect only: the one, which precipitates first, attaches itself to the sides of the vessel under the form of a yellow, thick, tenaceous fat; the other remains suspended in the liquor, under the form of scales, white and brilliant like boracic acid.

Sect. III.

Desiccation of the Brain.

Nine ounces, one gros (about 292 grammes, or 4312 grains troy, or very nearly three quarters of a troy pound), of cerebral matter, when dried over the water-bath, were reduced to two ounces, or nearly to a fifth part of their original weight; but the desiccation was not complete. These two ounces of matter, burnt in a platinum crucible, decrepitated and melted, and produced a smoke, which had the odour of an empyreumatic oil. This oil, in burning, gave a yellowish white and very large flame, and deposited a great deal of lamp black. Then the odour of the empyreumatic oil became imperceptible. As soon as the flame ceased the crucible was withdrawn from the fire. The charcoal which it contained weighed $5\frac{1}{10}$ grammes (1 gros, 24 grains; or 78·7 grains troy). It was reduced to powder, and exposed again to heat in a platinum crucible. Though exposed to a violent heat, it did not appear to burn; but softened, assuming a pasty form.

After having been exposed for an hour to a white heat, its weight was still 4·68 grammes ($72\frac{1}{4}$ grains troy); so that it had only lost 38 hundred parts of a gramme, which demonstrates a very difficult combustion in this charcoal.

Being washed with boiling water, and dried, it now weighed only 2·36 grammes (36·5 grains troy). Hence it had lost 2·32 grammes.

The solution strongly reddened the tincture of litmus; and the precipitate which lime-water formed in it was redissolved, till the excess of acid was saturated.

The same charcoal, exposed to heat a second time, burnt with a slight flame of phosphorus; but after a certain interval it softened as before, and assumed the form of a paste. It was washed a second time, and the water became acid, as before. These processes were repeated in the same manner till the whole of the charcoal was consumed.

The water employed in washing the charcoal being evaporated, yielded a white deposite, with a tint of blue, and a pasty consistence. This deposite, being separated from the liquor by the filter, melted very readily into a transparent glass. The same deposite reduced to powder, and mixed with diluted sulphuric acid, furnished sulphate of lime, but in a quantity which did not correspond with that of the matter employed.

Ammonia being mixed with a small portion of the liquid from which the above-mentioned deposite had been separated, occasioned only a very slight precipitation. Caustic potash, on the contrary, occasioned a very plentiful one. This last precipitate was chiefly magnesia, while the deposite formed spontaneously in the liquor was phosphate of lime.

As every thing seemed to show that the acidity of the liquor mentioned above was due to phosphoric acid, lime-water was mixed with it till no farther precipitation took place. This last precipitate being washed, was dissolved in muriatic acid, and the lime precipitated from it by means of oxalate of ammonia. The liquor of this last experiment was treated with caustic potash; but no precipitate took place while it remained cold. A boiling heat being employed, a flocky precipitate was obtained, which possessed the properties of magnesia.

The liquor precipitated by lime-water, as mentioned above, was evaporated in an open vessel, that the excess of lime might fall down. After filtration this liquid had a yellowish colour, a caustic taste, and precipitated abundantly muriate of platinum yellow. This liquor, when concentrated, was left in the open air, that it might crystallize, and that it might be seen whether it contained soda; but all the experiments to which it was subjected demonstrated that it was only potash partly saturated with carbonic acid.

These experiments on the combustion of the brain prove that the salts contained in that organ are phosphates of lime, of magnesia, and of potash.

The matter of the brain, after having been repeatedly boiled in alcohol, being burnt in a platinum crucible, exhibited almost the same phenomena as the brain in its natural state; that is to say, it decrepitated and flamed, but emitted less smoke; and its charcoal being calcined, did not soften, and gave no signs of acidity. This proves that the constituents which produced this effect in the entire brain were removed by the alcohol. We shall see hereafter what these constituents are.

Sect. IV.

Examination of the fatty matter of the brain which is deposited during the cooling of the alcohol in which brain has been boiled.

We have already described the principal physical characters of this substance: we have said that it was white and solid, but soft, and of a pitchy consistence; that it had a brilliant and satinlike aspect; and that it stained paper in the same manner as oils do. We shall now examine its chemical nature and composition.

1. When exposed to heat it melts, but it does not become so fluid as tallow does, and assumes a brown colour at a temperature at which common fat is not altered.

2. It dissolves in hot alcohol, leaving only a few flocks of animal matter, which had been dissolved in the first operation by means of the water contained in the brain. During the cooling of the alcohol the greatest part of this matter precipitates with all its usual characters: 20 parts of alcohol at 36 degrees are sufficient to dissolve one part of this matter.

3. When exposed to the sun this matter acquires a yellow colour, nearly similar to that of the fatty matter which is obtained by the evaporation of the alcohol, after it has deposited the fatty matter, the properties of which we are describing. I do not know the reason of this phenomenon.

4. A portion of this matter, which had been dissolved several times in alcohol to separate the last remains of the animal matter which it contained, was burnt in a platinum crucible. The combustion took place very readily, and was accompanied by a great deal of flame and smoke. The charry residue washed with distilled water communicated to that fluid a very distinct acidity, and the property of precipitating lime-water.

The singular result of this operation, which announced unambiguously the presence of phosphoric acid, made me suspect that this fatty matter contained phosphoric acid, or phosphate of ammonia, the base of which might have been volatilized by heat, though this last opinion was not very probable. However, to determine the point, I made the following experiments :—

1. I mixed the fatty matter with distilled water, and observed with surprise that it formed with that fluid a kind of emulsion, and did not separate from it. At the same time, I observed that this emulsion possessed no acid properties, and did not alter the colour of tincture of litmus.

2. I mixed it with a solution of caustic potash, and perceived

no indication of the presence of ammonia. Even a boiling heat did not develope the smallest trace of this alkali. In this experiment I was very much surprised to perceive, that though I had employed a quantity of potash more than sufficient to dissolve a quantity of tallow, more than I employed, yet the solution did not take place; and the mixture remained as milky as if water had been employed instead of potash.

I think we may conclude from these experiments that the fatty matter from the brain contained neither phosphoric acid nor phosphate of ammonia, and that the acid which appears after the combustion had another origin.

3. A hundred parts of the fatty matter of the brain were heated in a platinum crucible, with 200 parts of potash, and a little water. The mixture did not melt; but, on the contrary, became harder, which would not have happened if the substance in question had been real tallow. When the humidity was dissipated it assumed a brown colour, took fire, emitted an odour of burning grease, and gave out a great deal of smoke. The residue of this operation was washed with distilled water; the liquid being saturated with nitric acid, and boiled, gave, when mixed with lime-water, a flocky precipitate, which was phosphate of lime, and which weighed, when dry, the tenth part of the mass employed.

4. A hundred parts of the same matter thrown successively into melted nitre took fire with great facility, producing scarcely any smoke, the whole was destroyed, and not the smallest trace of charry matter remained. The residue of this operation, treated in the same manner as the preceding, gave the same quantity of phosphate of lime.

What conclusion can be drawn from these experiments, except that there is phosphorus combined with the fatty matter of the brain, and which dissolved in alcohol at the same time with the fatty matter? We find in the residue after combustion neither phosphate of lime nor phosphate of magnesia. The alkaline phosphates would have found enough of water in the brain to remain in solution in the alcohol, and not to precipitate when the liquid cools. Accordingly we find phosphate of potash, superphosphate of lime and of magnesia, in the residue of the alcohol evaporated, which had been digested with the cerebral matter. We must therefore admit the existence of phosphorus in the brain, as well as in the roes of fishes, where it was discovered by Fourcroy and me. The proportion of it, indeed, is very small; for from the quantity of phosphate of lime which I obtained in the preceding experiments, I estimate its quantity not to exceed $\frac{8}{100}$ths of a part: but if we subtract the humidity of the brain, and only consider the dry residuum, in that case the phosphorus may be considered as amounting to about $\frac{1}{100}$th part of the whole.

Though the substance whose properties have been described in this section has more analogy with tallow and fat than with any other class of bodies, yet it ought not to be confounded with ordinary fat. It differs from it principally by its solubility in alcohol, its capacity of crystallizing, its viscidity, its inferior fusibility, and the black colour which it assumes in melting. Thus, though we class it among fatty bodies, we ought to consider it as a particular and new species.

Sect. V.

Of the fatty matter of the brain which remains in solution in the alcohol after its cooling.

We have observed before, that after the alcohol digested with the brain had deposited its fatty matter, it remained of a green colour; and that the third, the fourth, and even the fifth portion of alcohol, which had been digested on the same portion of brain, had a sapphir blue colour. In order to discover the colouring matter we distilled this alcohol. The following are the observations that we made:—

The green and blue colour is not destroyed by the evaporation of the alcohol, as long as any of the alcohol remains; but as soon as the whole is driven off, the matter acquires a yellow colour, of more or less intensity. Neither the alkalies nor acids change these colours.

When these operations are performed on the first and second portions of alcohol which have been digested on the same quantity of brain, we see, as has been mentioned above, an oily fluid of a yellow colour precipitate itself to the bottom of the aqueous fluid derived from the humidity of the brain. But this effect does not take place with the last portions of alcohol, because they contain no more water.

The liquid, at the bottom of which this fatty matter collects, has likewise a yellow colour, a taste of the juice of meat, and slightly sweetish, and it gives marks of acidity. While this liquor is hot, the matter remains quite distinct, and seems to have some consistence; but by cooling, or on the addition of a little water, it absorbs humidity, becomes opake, and so mixed with the water that it cannot be separated. We must therefore take advantage of the favourable moment to make this separation in the proper manner.

From these remarks it is obvious that hot water must be employed to wash this substance, and to free it from the soluble matters with which it is mixed.

To dry this oil after washing it, we may expose it for some time to the open air, or to a gentle heat.

Let us examine the properties of this matter thus purified, leaving to another section the examination of the water from which it has been separated.

1. It has a reddish brown colour, an odour similar to that of the brain itself, but stronger. Hence it is probably this substance which gives its peculiar odour to the brain.

2. Its taste is similar to that of rancid fat.

3. When agitated with cold water it mixes with that liquid, and forms a sort of homogeneous emulsion, which separates only very slowly. The mineral acids, mixed in a certain quantity with this emulsion, immediately precipitate an oily matter, under the form of white opake flocks; and the liquor then passes clear through the filter, which was not the case before. The muriatic acid which has thus served to coagulate this species of emulsion, lets fall very light white flocks, when mixed with ammonia; but when nitric acid is employed, it neither can be made to precipitate by ammonia nor lime-water.

The infusion of nutgalls likewise coagulates this emulsion.

4. If the water be decanted off as soon as the fatty matter is deposited, and it be left to itself, it putrifies, and exhales a fetid odour, indicating the presence of an animal matter.

5. It dissolves in hot alcohol, some light flocks excepted, which do not amount to the hundredth part of it. The greatest part of it separates from the alcohol when it cools, and renders it milky, as would happen to a solution of resin.

6. Exposed on burning coals it melts, blackens, swells up, and emits an odour of burning animal matter, and afterwards that of grease in the state of vapour.

7. When burnt in a platinum crucible, either alone or mixed with potash or nitrate of potash, it always furnishes phosphoric acid, either uncombined or combined with the alkali, according to the process; just as happens to the fatty matter deposited from the alcohol during its cooling. Hence we must form the same opinion respecting the origin of this acid. We must admit the presence of phosphorus in the fatty matter.

From 400 grammes of brain employed in this process we have obtained about 3 grammes of this matter, which amounts to about 0·75 of a gramme per cent.

We ought now to inquire in what this substance differs from that which falls spontaneously from the alcohol during its cooling, the properties of which have been already described.

Though it remains in solution in the cold alcohol, it is not very soluble in that liquid; for when alcohol, at a boiling temperature, is saturated with it, a portion is deposited, as the alcohol cools, in the form of flocks. In this respect it approaches very near the first substance. It differs from it by its reddish brown colour, by its smaller consistence, by a slight taste of boiled meat which the first substance has not, and by a greater tendency to crystallization.

This difference is produced by a certain quantity of animal matter, of which we shall speak hereafter, and which may be separated from the fatty matter by means of cold alcohol.

Sect. VI.

Of the yellow aqueous liquor which remains after the separation of the two fatty substances, by cooling and by evaporating the alcohol.

When one has deprived the brain, by means of alcohol, of every thing soluble in that liquid, and separated, by the methods above described, the two fatty substances from the alcohol, there remains a liquor of a brownish yellow colour, which has the taste of the juice of meat with a little sweetness. This liquid reddens litmus; and is precipitated by lime-water, infusion of nutgalls, &c.

To learn the nature of the substances contained in that liquid we in the first place diluted it with a quantity of distilled water, and then poured into it lime-water as long as any precipitate continued to fall. The matter washed and dried in the open air had a yellow colour. When calcined it assumed a black colour, owing to the presence of a little animal matter, which is decomposed by the heat.

This substance thus calcined and redissolved in nitric acid was again precipitated white by ammonia. It was not blackened by exposure to heat, and possessed the characters of phosphate of lime.

After having precipitated, by means of lime, the phosphoric acid contained in the aqueous liquid, we evaporated it to dryness with the requisite precautions. The matter which it furnished weighed 4·5 grammes (69·5 grains troy). In this state it had a reddish brown colour, was semitransparent, had a taste similar to the juice of meat with a little sweetness; it dissolved in alcohol with great facility, leaving only some atoms of a saline matter which effervesced with acids.

Exposed to the air it became soft by attracting humidity. A portion of this matter being heated in a platinum crucible, swelled up considerably, and emitted vapours which had the odour of burning animal matter. It left a charcoal, which yielded, when washed with water and the liquid was evaporated, a little pure carbonate of potash.

It follows, evidently, from these experiments, that the aqueous liquid contained uncombined phosphoric acid and phosphate of potash, or perhaps superphosphate of potash and an animal matter, which by its solubility in alcohol and water, by its property of being precipitated by infusion of nutgalls, by its reddish brown colour, its deliquescence, its taste and smell of the juice of meat, ought to be regarded as identical with the substance which Rouelle formerly called the saponaceous extract of meat, and to which M. Thenard has given the name of *osmazome*.

It is, without doubt, this substance, a portion of which remains with the fatty matter obtained from the alcohol by evaporation, which gives it the reddish colour, the property of mixing with water, and of emitting the smell of animal matter when burning.

SECT. VII.

Statement of the constituents of the brain soluble in alcohol.

We now know the different substances separated from the brain by alcohol when repeatedly digested upon it. They are,

1. A fatty matter, white, solid, of a satin lustre, and a tenacity not to be found in ordinary tallow or fat.

2. Another fatty matter of a red colour, having less consistence than the preceding, but which seems to differ from it only in consequence of a little osmazome which remains mixed with it.

3. An animal matter of a reddish brown colour, soluble in water and alcohol, forming with tannin an insoluble combination, having the smell and taste of the juice of meat, and which is certainly the principle at present distinguished by the name of *osmazome*.

4. Superphosphate of potash, together with some traces of common salt, of which I have not spoken, because it occurs in all the animal humours.

SECT. VIII.

Examination of the part of the brain which is insoluble in alcohol.

When we have separated, by repeated digestions in boiling alcohol, every part of the brain soluble in that liquid, there remains a greyish white matter in the form of flocks, which has the appearance of fresh cheese, but differs from that substance by its chemical properties: 400 grammes of fresh brain furnished 31 grammes of this substance.

This substance in drying assumes a grey colour, a semitransparence, and a fracture similar to that of gum arabic.

Put into water in that state it absorbs a portion of it, becomes opake, swells up, and softens. The water dissolves a small portion of it, for it becomes putrid after an interval of some days.

Thus softened it dissolves readily by the assistance of heat in caustic potash, and during the solution no ammonia is disengaged, as is the case with the curdy portion of milk when dissolved in the same manner.

The solution of this substance in potash is slightly brown, its smell is not strong, the acids precipitate it in the form of white flocks, and disengage a very fetid odour. When acetate of lead is dropped into the solution, a dark brown precipitate falls, showing obviously the presence of sulphur.

Five grammes (77·2 grains troy) of this matter cautiously distilled, furnished carbonate of ammonia in crystals, and a red oil having a smell similar to that of albumen decomposed in the same manner. There remained in the retort 1 gramme (15·4 grains troy) of charcoal, which required 5 grammes of nitre to be entirely burnt.

The solution of salt obtained from it left 5 centigrammes (0·77 of a grain troy) of earthy residue, which was phosphate of lime. The liquid being supersaturated by nitric acid, and subjected to ebullition, let fall no precipitate; but it yielded a copious one when mixed with lime-water. This shows that the phosphate of magnesia had been decomposed by the potash, and perhaps even a portion of the phosphate of lime.

This matter heated alone in a crucible, decrepitates, swells, and melts like albumen. Its charcoal, though calcined for a long time, does not become acid like that of the fatty matter; which shows that it contains no phosphorus. This charcoal, being washed with muriatic acid, furnished a small quantity of phosphate of lime and phosphate of magnesia.

When thrown into melted nitre it burns rapidly, and with flame; and we find in the alkali resulting from that operation very sensible traces of sulphuric acid, though the saltpetre employed contained none of it. This proves that the matter of the brain which is insoluble in alcohol contains sulphur; and confirms what was indicated by the acetate of lead dropped into the alkaline solution of this substance.

The properties, which the portion of the brain insoluble in alcohol has presented, leave no doubt that it is perfectly identical with albumen. The knowledge of this circumstance explains very well the coagulation of the brain mixed with water by heat, acids, metallic salts, &c. This was the opinion which Fourcroy had formed of this substance in his memoir on the subject published in the Annales de Chimie.

Sect. IX.

General Result.

The mass of the brain, then, is composed of the following substances:—

1. Two fatty matters, which are probably identical.
2. Albumen.
3. Osmazome.
4. Different salts; and among others, phosphates of potash, lime, and magnesia; and a little common salt.
5. Phosphorus.
6. Sulphur.

I conceive, as far as it is possible to draw conclusions from experiments so delicate, that these substances exist in the brain in the following proportions:—

1. Water	80·00
2. White fatty matter	4·53
3. Reddish fatty matter	0·70
4. Albumen	7·00
5. Osmazome	1·12
6. Phosphorus	1·50
7. Acids, salts, and sulphur	5·15
	100·00

Sect. X.

Putrefaction of the cerebral matter.

A portion of brain being diluted with a certain quantity of water, and abandoned to itself during a month, presented the following phenomena. At first it separated into three parts. That portion which occupied the surface was a part of the matter of the brain elevated by air bubbles attached to it. The portion in the middle was a yellow coloured liquid, which after an interval of some days assumed a fine red colour, which it retained for more than 20 days. After that period this colour by degrees faded, and was succeeded by a yellow colour, more intense than that which the liquid had formerly possessed. The third portion, occupying the bottom of the vessel, was another part of the matter of the brain. During the month that it was allowed to remain it emitted no gaseous matter.

The vessel containing this mixture being left open, there issued out of it an invisible vapour, having a disagreeable smell, somewhat similar to that of putrid cheese. Some persons compared it to the odour of the intestines when beginning to decompose.

A paper dipped in the solution of acetate of lead being exposed to this vapour assumed immediately a blackish brown colour.

The liquid in which the brain had thus putrified was sensibly alkaline; at least it restored the colour of litmus paper reddened by acids, and formed white vapours when oxymuriatic acid was brought into its neighbourhood.

The liquid separated by filtration from the matter of the brain had an amber colour. Acids rendered it muddy, throwing down white flocks. The odour which it exhaled in these circumstances was more fetid and disagreeable than before. Oxymuriatic acid rendered it muddy, likewise; but at the same time entirely destroyed its odour.

After being filtered the liquor was subjected to distillation. As soon as it was heated nearly to the boiling temperature, yellow flocks separated in abundance, as happens when a diluted solution of albumen is treated in the same manner.

The product of the distillation was without colour. Its odour was perfectly similar to that of the liquid before distillation. It precipitated acetate of lead white, and restored the colour of litmus reddened by an acid. Oxymuriatic acid destroyed its odour, and made it assume a yellow colour.

When the liquor remaining in the retort was reduced to about a fifth part, it was filtered. Its colour was yellow, and its odour similar to that of old cheese. It had become acid, for it reddened the colour of litmus paper. Infusion of nutgalls, lime-water, and alcohol, formed flocky precipitates in it. Ammonia also occasioned a granular and semitransparent precipitate, which resembled ammoniaco-phosphate of magnesia. Concentrated sulphuric acid being mixed with this liquid developed a strong smell of vinegar.

The solid matter of the brain which had undergone fermentation, being washed with water, and submitted to the action of alcohol, communicated to it a bluish green colour, as if the brain had undergone no alteration. This alcohol, on cooling, deposited a white matter, partly in flocks, partly in crystals. There remained a greyish substance, which the alcohol had not dissolved, and which resembled albumen.

From these experiments we may conclude,

1. That the fatty portion of the brain had undergone no sensible change during the putrifaction of this organ. It preserved the property when dissolved in alcohol to give it a green colour, and to precipitate, on cooling, in a crystalline form, and retaining all its properties.

2. That a part only of the albumen was destroyed by the fermentation, that from this decomposition a small quantity of ammonia resulted which dissolved another portion of the albumen, and some acetic acid rendered sensible by the addition of sulphuric acid.

3. That the osmazome was not decomposed, at least completely, since its presence was still recognised in the concentrated liquid.

We conceive that the albumen of the brain putrifies much more speedily, and undergoes a more complete alteration, when it is in contact with the air than when it is confined in a close vessel.

I do not know what the substance is which assumes a red colour during the putrifaction of the brain. I thought at first that it was the substance which gives a green colour to alcohol; but I gave up that opinion when I saw that the cerebral matter still communicated the same colour to alcohol after its putrifaction.

The cerebellum of man, and the brain of herbivorous animals, being examined in the same manner, and with the same precautions, yielded the same results. I propose to continue these researches on the brain of other classes of animals.

Sect. XI.

Of the medulla elongata, and spinal marrow.

The medulla elongata and spinal marrow are of the same nature as the brain; but they contain much more fatty matter, and less albumen, ozmazome, and water. Hence the reason why the spinal marrow has greater consistence than the brain.

The spinal marrow communicates to alcohol, when boiled in it, a blue colour, as the brain does. It contains, likewise, superphosphate of potash. The portion insoluble in alcohol is of the same nature as that of the brain; that is to say, albumen. The fatty matter contains phosphorus, like that of the brain.

Of the nerves.

The nerves are likewise of the same nature as the brain; but they contain much less fatty matter, and green colouring matter, and much more albumen. They contain, besides, common fat, which separates from them when treated with boiling alcohol, and deposites itself at the bottom of that liquid.

The nerves deprived as much as possible of their fatty matter by means of alcohol become semitransparent. Treated for a long time in that state with boiling water, they do not dissolve; but become white, opake, and swell up, obviously in consequence of absorbing moisture. The water in which they were boiled holds in solution a small quantity of matter; for the infusion of nutgalls forms a precipitate in it, and the solution properly evaporated yields a little jelly, derived probably from the cellular texture which binds the nervous fibres together.

After having been treated with alcohol and with water, the nerve dissolves almost completely in caustic potash. Only a few flocks remain, not amounting to the hundredth part of the mass employed. No ammonia is produced during the solution.

The solution of nerve in alkali is precipitated by acids, and the precipitate, as well as the liquid from which it fell, assumes a purple colour.

Nerve preserved in water undergoes little alteration. The water, however, after a few days, assumes the odour of semen very sensibly.

Nerve put into oxymuriatic acid contracts its dimensions. As it is chiefly the envelope of the nerves which undergoes this change, the nervous substance issues from its case, and each of the fibres which compose it separates from those in its neighbourhood: so that the nerve looks like a hair-pencil with its extremities diverging. In this situation the substance of the nerve assumes more consistence and whiteness, owing to the condensation and opacity which it requires. From this experiment, it would seem that oxymuriatic acid would furnish a good instrument for facilitating the study of the nerves and their envelope.

Is it possible, from the experiments to which the brain has been subjected, to determine the state in which each of the elements composing this organ exists in it? Is not the albumen united to a portion of phosphoric acid, and is not its consistence and opacity owing to this combination? Without affirming any thing on this head, I will say, that this substance appears to have acquired its state of semicoagulation from an acid; just as happens to the curdy part of sour milk; and that this coagulation is produced entirely by a fermentation, which commences like that of milk, by being acid.

I next proposed to myself this question. Is the fatty matter in combination with the albumen and the osmazome? This seems to be the case, at least with regard to the fatty matter and albumen; for when the matter of brain is triturated with water, and converted into a species of emulsion, if it be left at rest the albumen and fatty matter separate together, and the osmazome remains in solution in the liquid, together with a small portion of the albumen. At the same time, I acknowledge that it is possible that these two substances are only in the state of mixture, and that the albumen here performs the same office to the fatty matter that mucilage* does to the oils of emulsive seeds.

* I call mucilage, with all the chemists, the substance which holds the oil in suspension in the emulsion of almonds, though it be of a very different nature from gum.

Article III.

Population of France in 1812. *Copied from the French Exposé for* 1812. *Published in the Moniteur of Feb.* 27, 1813.

I. Old France.

	Departments.	Population.	Size in Square Miles.
1	Ain	304,468	1518·0
2	Aisne	442,989	2069·0
3	Allier	260,266	2050·0
4	Lower Alps	146,994	2057·7
5	Higher Alps	124,763	1529·0
6	Ardeche	290,833	1519·0
7	Ardennes	275,792	1450·8
8	Arriege	222,936	1462·5
9	Aube	238,819	1686·5
10	Aude	241,993	1796·7
11	Aveyron	331,373	2272·8
12	Bouche du Rhone	293,235	1662·6
13	Calvados	505,420	1571·9
14	Cantal	251,436	1585·6
15	Charente	326,885	1626·3
16	Charente, Infer.	393,011	1980·0
17	Cher	228,158	2044·0
18	Correze	254,271	1642·6
19	Corsica	174,702	2708·0
20	Cote-d'or	355,436	2422·0
21	Côte du Nord	519,620	2034·8
22	Creuse	226,224	1600·4
23	Dordagne	424,113	2481·0
24	Doubs	226,093	1466·6
25	Drome	253,372	1866·8
26	Eure	421,481	1832·0
27	Eure et Loire	265,996	1679·0
28	Finisterre	452,895	1955·1
29	Gard	322,144	1656·4
30	Garonne (Haute)	367,551	1774·7
31	Gers	286,497	1800·6
32	Gironde	514,462	2900·0
33	Herault	301,099	1742·6
34	Ille et Villaine	508,344	1883·6
35	Indre	204,721	1899·6
36	Indre et Loire	275,292	1720·9
37	Isere	471,660	2323·5

Departments.	Population.	Size in Square Miles.
38 Jura	272,883	1390·3
39 Landes	240,146	2487·3
40 Loir et Cher	213,482	1663·0
41 Loire	315,858	1331·4
42 Loire (Haute)	263,202	1388·9
43 Loire, inferieur	407,827	1950·7
44 Loiret	285,395	1864·8
45 Lot	868,149	1467·0
46 Lot et Garonne	326,127	1471·1
47. Lozere	143,247	1407·4
48 Maine et Loire	404,489	1985·3
49 Manche	581,429	1866·3
50 Marne	311,017	2265·6
51 Marne (Haute)	237,785	1748·8
52 Mayenne	232,253	1433·1
53 Meurthe	365,810	1737·3
54 Meuse	284,703	1669·4
55 Morbiham	403,423	1882·8
56 Moselle	385,949	1742·4
57 Nievre	232,263	1896·4
58 Nord	859,833	1597·6
59 Oise	383,507	1605·9
60 Orne	425,920	1783·3
61 Pas-de-Calais	570,338	1877·3
62 Puy-de-Dôme	542,834	2194·1
63. Pyrenées (B.)	383,502	2087·9
64 Pyrenées (H.)	198,763	1294·9
65 Pyrenées-or	126,626	1136·2
66 Rhin (Bas)	500,926	1368·8
67 Rhin (Haute)	414,265	1518·0
68 Rhone	340,980	746·9
69 Saone (Haute)	300,156	1262·1
70 Saone et Loire	471,457	2370·0
71 Sarthe	410,380	1765·6
72 Seine	630,636	139·4
73 Seine, Infer.	642,948	1640·1
74 Seine et Maine	304,068	1646·1
75 Seine et Oise	430,972	1588·3
76 Sevres (Deux)	254,105	1616·5
77 Somme	495,058	1669·5
78 Tarn	295,885	1593·2
79 Tarn et Garonne	230,514	1026·5
80 Var	283,296	1451·6
81 Vendée	268,746	1865·6

Departments.	Population.	Size in Square Miles.
82 Vienne	253,048	1903·3
83 Vienne (Haute)	243,195	1574·4
84 Vosges	334,196	1623·9
85 Yonne	325,994	2014·1
Total	28,786,911	147,973·0

II. *Countries added to France since* 1789.

1 Alpes Maritims	131,266	890·1
2 Appenins	238,624	1500·1
3 Arno	538,450	2354·2
4 Bouches de l'Elbe	375,977	2040·5
5 Bouc. de l'Esc.	76,315	174·0
6 Bouc. de la Mse.	393,081	1044·8
7 Bouc. du Rhin	257,573	1134·8
8 Bouc. du Weser	331,030	2809·7
9 Bouc. de l'Yssel	144,433	939·1
10 Doire	234,822	692·8
11 Dyle	431,969	946·9
12 Ems Occidental	191,094	1418·5
13 Ems Oriental	127,959	878·4
14 Ems Superieur	420,291	2717·0
15 Escaut	636,438	797·8
16 Forêts	146,333	1908·6
17 Frise	175,350	496·7
18 Gênes	400,056	656·2
19 Jemmappe	472,366	1040·3
20 Leman	210,478	773·3
21 Lippe	137,750	1569·4
22 Lys	491,143	1013·4
23 Marengo	318,447	961·9
24 Mediterranée	268,368	1356·1
25 Meuse Infer.	267,249	1045·8
26 Mont Blanc	300,239	1768·8
27 Montenotte	289,823	1097·1
28 Mont Tonnère	428,988	994·2
29 Nethys (Deux)	284,584	788·2
30 Ombrone	151,250	2169·8
31 Ourthe	352,264	1203·5
32 Po	399,237	1144·9
33 Rhin et Moselle	249,010	1625·2
34 Roer	621,410	1441·7
35 Rome	548,909	1015·5
36 Sambre-Meuse	180,655	1264·8

Departments.	Population.	Size in Square Miles.
37 Sarre	273,569	1363·1
38 Sesia	202,822	1428·5
39 Simplon	63,533	1381·0
40 Stura	431,438	3112·8
41 Taro	352,214	1004·3
42 Trasimene	300,709	2264·2
43 Vaucluse	205,832	647·8
44 Yssel superieur	192,670	1549·7
45 Zuyderzee	505,387	2624·2
Total	13,951,466	61049·7

Old France	28,786,911	147,973
Usurped Countries	13,951,466	61,049·7
Total	42,738,377	209,022·7

Inhabitants to the Square Mile.

In Old France .... 194·5
In the Usurped Countries .... 228·5

The population of England is 196·3 persons to the square mile; so that it is more populous than Old France; but much less so than the usurped countries, which consist of the Low Countries and portions of Italy, by far the best peopled portions in Europe.

Article IV.

On Veins. By Thomas Thomson, M.D. F.R.S.

It is generally known that the globe of the earth, as far as its structure has been examined, is composed of rocky masses, which, it would seem, extend quite round the globe of the earth, enclosing it somewhat like the coats of an onion. These rocky masses are composed in some cases of a single mineral; as *limestone, serpentine, quartz*; in others, two or more minerals are mixed together in the same rock. Thus *granite* is composed of *felspar, quartz,* and *mica*; and *greenstone* of *felspar* and *hornblende.* All the different rocks distinguished by a name amount to about 50; and as far as observation has yet gone they lie over each other in regular order. Werner, to whom we are indebted for the first classification of rocks, has divided them into four distinct classes, beginning with the lowest, and terminating with the highest. To these classes he has given the name of formations. They are as follows:—

I. *Primitive Formations.*

Principal.	Subordinate.
1. Granite.	5. Older porphyry.
2. Gneiss.	6. Primitive trap.
3. Mica slate.	7. Primitive limestone.
4. Clay slate.	8. Older serpentine.
	9. Quartz.
	10. Gypsum.
	11. Older flinty slate.
12. Newer porphyry.	
13. Syenite.	
14. Newer serpentine.	

II. *Transition Formations.*

1. Grey wacke.
2. Transition limestone.
3. Transition trap.
4. Transition flinty slate.

III. *Floetz Formations.*

1. Old red sandstone.
2. First floetz limestone.
3. First floetz gypsum.
4. Variegated sandstone.
5. Second floetz gypsum.
6. Second floetz, or shell limestone.
7. Third sandstone, or freestone.
8. Chalk.
9. Independent coal.
10. Floetz trap.

IV. *Alluvial Formations.*

Sand, gravel, &c.
Loam.
Clay.
Turf, &c.

These rocks are sometimes composed of immense blocks, sometimes of regular layers placed one above the other. When these layers consist of repetitions of the same kind of rock, they are called *strata*; when they consist of different kinds of rock, they are called *beds*. The position of these strata and beds is very various. In some cases they are nearly vertical; in others, perfectly horizontal; and they are to be found at all angles between the vertical and horizontal.

Besides these rocks there is another species of solid matter which frequently occurs in the earth, small indeed in point of quantity, but of the utmost importance in a mineralogical point of view, in consequence of the curious information with which it furnishes us. The rocks are occasionally intersected by matter of a different kind, which passes through them in a vertical direction, or nearly so. These intersections are called *veins.* We shall have a tolerably precise notion of the appearance of a vein, if we suppose that the rock or mountain in which it occurs was by some means or other cleft in two from top to bottom, and that the rift has been afterwards filled up with stony matter. This *stony matter* constitutes the *vein.* When the rock in which the vein occurs is stratified, the veins always cut through the strata.

By far the best account of veins hitherto given to the public is contained in a treatise on the subject published by Werner, of Freyberg. It was not drawn up, as he himself informs us, till he had himself examined several thousand veins, till he had seen specimens from several thousand more, and till he had analysed all the correct accounts of the structure of veins, in every country which it was possible to procure. This book, it must be confessed, is neither elegantly written nor well arranged; but it contains a vast collection of accurate and important facts. There is both a French and an English translation, the last of which is the best.

I shall state here a few of the leading facts respecting veins that appear completely established.

1. Veins occur in every species of rock, and in every formation; unless some of the alluvial formations be considered as exceptions: but they diminish both in variety and importance, according to the order of the formations.

2. They vary in thickness, from 18 feet, which is the thickest vein that has hitherto been accurately described, to the 12th part of an inch, or even less. Their most common thickness is less than six feet, and they seldom exceed that standard.

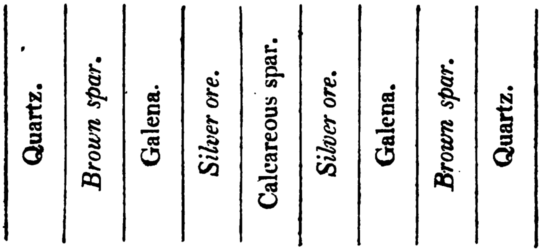
3. They are usually widest above, and become gradually narrower below, precisely as if they had been originally rents in the rock in which they occur: and the same vein often varies considerably in its thickness in different parts of its course.

4. Sometimes veins are filled up with one single kind of stony matter. Thus we have veins composed of *quartz*, of *limestone*, of *felspar*, of *greenstone*, &c. But it is common to find a variety of substances in the same vein. Thus we find limestone, sulphate of barytes, and grey copper ore, in the vein at Airthrey, near Stirling. In some veins more than 20 different substances occur together.

5. When the veins are narrow it is more common to find them of one substance; but when they are wide, various substances usually occur together in them. Now the structure of these *complicated veins*, if I may so term them, deserves particu-

lar attention. All the different substances are deposited in layers or plates parallel to the sides of the vein. And they follow a determinate order. A particular layer constitutes the middle of the vein; and the very same substances in the very same order are repeated on both sides of it to the walls of the vein. An example will make this structure evident.

I shall select for that purpose a vein described by Werner in his treatise. It occurs in a gneiss rock near Freyberg, and consists of nine layers, or four pairs, regularly arranged on each side of the central one; and each pair is composed of the same substance, and has the same thickness. Next the walls of the vein on both sides, there is a layer of quartz; next to the quartz is a layer of brown spar; then follows a layer of galena, with a layer of silver ore; and the centre of the vein consists of a seam of calcareous spar. Thus the position of the layers is as in the following diagram:—



When these complicated veins are traced downwards, it sometimes happens that the central seam disappears altogether. In that case the two seams on each side of it coalesce into one, and become henceforth the central seam of the vein. If we continue our progress downwards, this new central seam may perhaps disappear in its turn, and the two layers on each side of it coalesce, and form a new central seam. This successive disappearing of the seams continues as we proceed downwards, till at last none remain; except the layers which are attached to the walls of the vein. These coinciding, form henceforth a simple vein, which gradually becomes thinner and thinner, and at last disappears altogether, and the vein terminates.

It is true that only a very few well authenticated instances of very complicated veins, that have been thus traced, can be produced; but a good many veins, consisting of three layers, have been wrought till the central seam disappeared, and the layers next the walls united, and formed a single vein. The layers next the walls of the vein are usually thinnest above, and become thicker as we descend.

6. It is no uncommon thing to find two or more veins in the

same rock not parallel to each other; but crossing and cutting through each other at various angles. When that happens, one of the veins always passes on without interruption, while the other is cut in two, and the parts of it separated from each other. Now it is impossible to avoid concluding, from the whole phenomena of veins, that the rocks in which they occur existed before them. It follows, irresistibly, from the fact just mentioned, that the veins themselves were not formed all at once, but at different periods; and the way in which they cross furnishes us with the key to determine their relative ages. That vein which passes without interruption through another must be *newer* than that other; and the vein which is interrupted, and its parts separated, must be the *older* of the two.

7. Every substance has been found in veins, which constitutes a part of all the various rocks composing the crust of the earth. The rocks themselves, each in its turn, fill various veins. On the other hand, every substance hitherto found in veins has been found also in rocks. Hence, with respect to constituents, there is no difference between veins and rocks.

8. All the rocks which constitute the crust of the earth, and which have been described and named by mineralogists, with scarcely a single exception, occur also in veins. Now it deserves particular attention that the veins filled with rocks are just as regular in respect to position as the rocks themselves, with portions similar to which they are filled. The order which they observe is this. Every rocky vein (if I may be allowed the expression) occurs in rocks nearer the central nucleus than the formation of which it constitutes a part. Or, in other words, veins filled with a particular formation are always found *under* that formation, and never above it. Thus in the *primitive* rocks we find veins of *transition* and *floetz* rocks; but in the *transition* rocks, we never find veins of any *primitive* formation, but only of *transition* and *floetz* rocks; and, lastly, in the *floetz* formations we never find veins either of *primitive* or *transition* rocks, but only of *floetz* rocks. The same invariable rule holds good with respect to the individual formations of all the great classes.

Farther, when two veins of different formations intersect each other, that which is filled with the formation nearest the nucleus is always *cut through* by the other.

The veins then follow the same invariable order with respect to position that the rocks do, and there is clearly a connection between them; but we know, from the facts already stated, that the veins must have been formed at different times. Now as the order in which they intersect each other is precisely the order of the position of the different formations, it follows irresistibly that the order of position is likewise the order of the formation of all the different rocks which compose the crust of the earth.

Here, then, we come to the knowledge of a fact of the utmost importance, that the rocks have been deposited in succession, that those nearest the central nucleus have been deposited first, and the others in the order of their position. Hence it follows that the language of the Wernerian geognosy is not hypothetical, since the *formations* must actually have been formed in the order of the classes; the *primitive* first, the *transition* next, the *floetz* next, and the *alluvial* last of all. The same holds good with the individual formations. Hence the terms *older* and *newer*, as applied to position, are perfectly correct. Granite is demonstrably the oldest, and floetz trap the newest, of the rocks.

9. Besides the rocky veins, many others occur not precisely similar to any beds hitherto discovered in the great mass of rocks; though it is neither impossible nor improbable that such beds may exist. Considerable progress has been made in classifying these veins, which is the first step towards an accurate knowledge of them. When veins are composed of the *same* constituents disposed in the *same* order, they are called veins of the *same* formation. It has been already shown that several very complicated veins, exactly similar in every particular, occur in countries at great distances from each other, clearly indicating a correspondence in their formation.

In this paper I have thrown together some of the most important facts respecting veins, in order to draw the attention of the geognosts of this country to the study of them. They constitute the phenomena from which by far the most important data, relative to the way in which the crust of the earth has been formed, may be drawn. In some subsequent essays, which I shall occasionally insert in this journal, I shall throw together the documents by which the truth of the most important of the preceding facts has been ascertained.

Article V.

An Account of the dreadful Accident which happened at Felling Colliery, near Sunderland, on May 25th, 1812. Extracted from an introductory account prefixed to The Funeral Sermon preached on the occasion, and published, by the Rev. John Hodgson.

(With a plan of the Colliery.)

Felling is a manor in the chapelry of Heworth, and parish of Jarrow, about a mile and a half east of Gateshead, in the county of Durham. It contains several strata of coal, the uppermost of which were extensively wrought in the beginning of the last century. The stratum called the High-main, was won in 1779, and continued to be wrought till the 19th January, 1811, when it was entirely excavated.

The present colliery is in the seam called the Low-main. It commenced in October, 1810, and was at full work in May, 1811. Messrs. John and William Brandling, Henderson, and Grace, have each a fourth share, both in its royalty and in the adventure: they have also a lease from the Dean and Chapter of Durham, of a large extent of coal, lying on the south and east of the manor of Felling.

The working or down-cast shaft, marked A on the annexed plan, is called the *John Pit,* and is situated on the north side of the Sunderland road, and half way between Felling Toll-bar and Felling Hall. It is 204 yards deep, and furnished with a *machine* or steam-engine for drawing the coal, and with an engine called a *whim gin,* wrought by horses, and of use in letting down and drawing up the workmen, when the machine chances to be crippled, or repairing: and when it lies idle on pay Saturdays and on Sundays. Here is also a high *tube* of brick-work, employed in assisting ventilation while this shaft was sinking, and till the communication by the narrow boards and the drifts was opened between the two shafts: since that it has been of no use.

The up-cast, or air furnace shaft, is called the *William Pit.* It is on an eminence 550 yards south-west of the John Pit, and is distinguished by a whim gin and a lofty tube of brick-work. This shaft is 232 yards deep.

Over each pit two iron pullies were suspended on a kind of scaffold, called the *shaft-frame.* In these ran the ascending and descending ropes. The pullies over the John Pit were six feet in diameter, and weighed nine cwt. a-piece. Those in which the rope of the gin of the John Pit ran, were fixed on a crane, which turned them over or from the shaft as occasion required.

As there are no feeders of water in the strata below the high main, the low main coal is kept perfectly dry by *tubbing* the watery seams with a circular casing of oak wood, formed into pieces resembling the fellies of a wheel: this contrivance has the appearance of the ashlar work of a well, and saves the expense of a steam-engine for drawing water. The white lines on the plan represent the excavated parts: the broadest of them are called *boards,* and those that cross them at right angles are *walls.* The two narrow lines which run north and south, on the east side, are called *double winning head-ways,* and the narrow lines between them, *stentings:* the two lines on the west side of the William Pit are also double winning head-ways. The two boards on the north are termed *the narrow boards:* they were the parts first excavated, and were made for the purpose of opening a communication for the atmospheric air between the two pits: the lines between the west end of the narrow boards and the William Pit, are called *drifts.* The *inclined plane board* is marked P P on the plan.

The parallelograms formed by the boards and walls, are called *pillars:* they are solid masses of coal left to support the roof of the mine, and are each 26 yards long, and eight yards broad.

The single black lines in the walls and stentings represent *stoppings,* and the double lines *trap-doors,* each of which are placed to divert the current of atmospheric air through proper channels. The stoppings are made of brick and lime; and in this colliery, were strengthened on each side with a wall of stone. The trap-doors are made of wood: each of them is attended by a boy about seven, eight, or ten years old; and they are seldom used but in the avenues leading from the working shaft to the workings. At the circle N, the air crossed the waggon-way, and at M, the way to the stable, over arches of brick. The walls which have stoppings in them, are called *sheth-walls,* and those that are open, *loose-walls.*

In all large collieries the air is accelerated through the workings, by placing a large fire, sometimes at the bottom, and sometimes at the top of the up-cast shaft, which in these cases is covered over and connected with a *furnace tube* or chimney, by an arched gallery of brick from 40 to 60 feet in length. In this colliery the furnace was about six feet from the bottom of the tube.

The first *course of the air,* after descending the John Pit, was under the arch M, up the inner narrow board and the stable board S, to the trap-door at the head of the narrow boards; then down the board next south of the stable board; and so afterwards up two boards and down other two, till it traversed the newly formed *sheth* or set of workings, branching from the southernmost part of the double-headways on the east: from thence it passed over the two arches up the outer board of the narro v boards, to the most westerly sheth of boards, and after fanning them, found its way down the crane board, along the drift to the William Pit, through which it ascended into the furnace, and thence, charged with noxious vapours, into the open air.

From this explanation it will easily be perceived that the purity and wholesomeness of a coal-mine has no reference to its depth. If the air be conducted through all parts of a mine, as here described, and no falls from the roof occur to prevent its visiting every corner, the old excavations, which are called *wastes,* will be constantly ventilated by as pure air as the boards in which the men are at work—each part of the mine will be uniformly wholesome; but when obstructions occur, and are not speedily removed; when the fire in the furnace shaft is neglected; or when care has not been taken to place the stoppings and trap-doors in proper places, or the trap-doors are carelessly left open, or stoppings fall down,—in all these cases accumulations of *fire-damp** (called *stythe* by the colliers), immediately commence in

* Note by the Editor.—What is called *fire-damp* in coal-mines is the *carbureted hydrogen gas* of chemists, as I have ascertained by direct experiments. It is composed of

Carbon	72
Hydrogen	28
	100

or of two atoms of hydrogen, and one of carbon. I have been informed that it always exists in coal-mines, mixed with carbonic acid; and all the specimens of it which I have ever procured for the purpose of examination, contained a mixture of that gas. Hence I conceive that fire-damp is formed by the action of coal upon water. The water is decomposed, two atoms at once. All the oxygen combines with carbon, and forms carbonic acid; while all the hydrogen unites likewise with carbon, and forms carbureted hydrogen, or fire-damp.

I never could succeed in making any mixture of fire-damp and common air explode. It only burnt rapidly, with a blue flame, and little noise; but when mixed with oxygen gas in the proper proportion it explodes with great violence. Suppose we take 100 measures of pure carbureted hydrogen gas, it will not explode unless the oxygen present amount to 105 measures, and it ceases to explode whenever the oxygen amounts to more than 227 measures. Hence it would seem that whenever the fire-damp in mines amounts to $\frac{1}{12}$th of the bulk of common air present, it will be apt to explode with a candle; and that whenever it exceeds $\frac{1}{6}$th of the air, it will no longer be capable of exploding. All proportions between $\frac{1}{6}$ and $\frac{1}{12}$ will explode.

We are not acquainted with any means of preventing the formation of this gas; but it certainly might be prevented from accumulatlng, by ventilating the mine properly. If the usual method of fires, &c. be insufficient, nothing would be easier than to pump the air out of the mine, by means of an engine; and this would secure a perfect ventilation at all times, unless we suppose the workmen culpably negligent. I would advise the overseers of coal-mines, where fire-damp exists, to learn the method of analysing the air of the mine, in order to know when the fire-damp approaches to $\frac{1}{12}$th of the air, that they might be aware of their danger, and have it in their power to take the requisite precautions to prevent it. The process to be followed is very simple: it would not require any expensive apparatus, and might be perfectly learned in two or places deprived of the atmospheric current, and continue to train their dreadful artillery, and grow strong in danger, till the *waste-men,* or ventilators of the mine, discover them, and wash them off, or they ignite at the workmen's candles. Blasts occurring in partial stagnations, as in the face of one or two boards, though they generally scorch the persons in their way, seldom kill them; but when the air has proceeded lazily for several days through a colliery, and an extensive magazine of fire-damp is ignited in the wastes, then the whole mine is instantly illuminated with the most brilliant lightning—the expanded fluid drives before it a roaring whirlwind of flaming air, which tears up every thing in its progress, scorching some of the miners to a cinder, burying others under enormous heaps of ruins shaken from the roof, and, thundering to the shafts, wastes its volcanic fury in a discharge of thick clouds of coal dust, stones, timber, and not unfrequently limbs of men and horses.

But this first, though apparently the most terrible, is not the most destructive effect of these subterraneous thunderings. All the stoppings and trap-doors of the mine being blown down by the violence of the concussion, and the atmospheric current being for a short time entirely excluded from the workings, those that survived the discharge of the fire-damp, are instantly suffocated by the *after-damp,* which immediately fills up the vacuum caused by the explosion.

This *after-damp* is called *choak-damp* and *surfeit* by the colliers, and is the carbonic acid gas of chemists. While the mine is at work, it lies sluggishly upon its floor, and suffers the atmospheric air, as a lighter fluid, to swim upon it: fire-damp being the lightest of the three, floats upon the atmospheric air, and therefore occupies a space, according to its present quantity, nearest the roof of the mine.

The coals from the boards on each side of the William Pit, were conveyed in strong wicker baskets called *corves,* to the crane, on *trams,* a narrow frame-work of wood mounted on four low wheels: this work was done by *barrowmen* and *putters,* some of whom are men, and manage a tram singly, by going behind it and pushing it forward; these are called *hewing*

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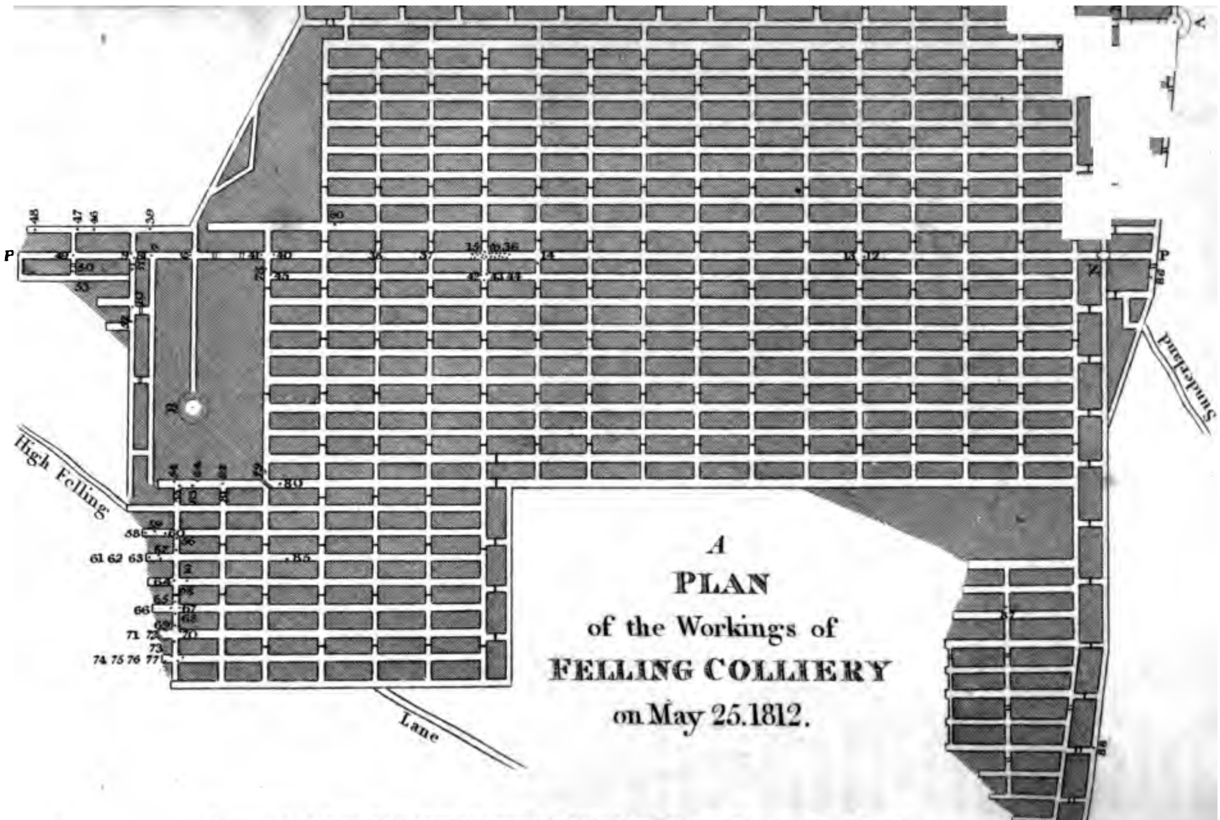
three days by any intelligent person. To attempt to describe that method here would serve no purpose; but it would give me great pleasure to explain it to any mine-masters or overseers who wish to prevent such disasters. I have not the least doubt that by a daily experiment, which would not occupy more than a quarter of an hour, all accidents from fire-damps might be effectually guarded against.

putters or *headsmen:* the others are two to a tram, and are called *headsmen* and *foals,* the former of whom pull before at a rope they call a *soam,* and the latter push behind with their shoulders: boys about 15 or 16 years old are employed in this department of the colliery. The *crane,* at the time of the accident, stood 11 pillars up the crane-board: it had been removed from the several pillars which have their uppermost corner canted off, and a period fixed in the vacancy. The use of the crane is to lift the loaden corves off the trams, upon waggons which differ little from the trams, except in their being larger and stronger. From the crane, about four waggons, each carrying two corves and chained together, were taken to the bottom of the crane-board near number 86, by the machine, called an *inclined plane,* which draws up the empty waggons by the weight of the loaded ones: the person who regulates this machine is called a *brake-man.* From the bottom of the inclined plane, the coals were conveyed on the same waggons to the John Pit.

This mine was considered by the workmen a model of perfection in the purity of its air, and orderly arrangements—its inclined plane was saving the daily expense of at least 13 horses—the concern wore the features of the greatest possible prosperity, and no accident, except a trifling explosion of fire-damp, slightly burning two or three workmen, had occurred. Two *shifts* or sets of men were constantly employed, except on Sundays. Twenty five acres of coal had been excavated. The first shift entered the mine at four o'clock A. M. and were relieved at their working posts by the next at 11 o'clock in the morning. The establishment it employed under ground, as will be seen in the succeeding narrative, consisted of about 128 persons, who, in the fortnight from the 11th to the 25th of May, 1812, wrought 624 scores of coal, equal to 1300 Newcastle chaldrons, or $2455\frac{20}{36}$ London chaldrons.

About half past eleven o'clock on the morning of the 25th May, 1812, the neighbouring villages were alarmed by a tremendous explosion in this colliery. The subterraneous fire broke forth with two heavy discharges from the John Pit, which were, almost instantaneously, followed by one from the William Pit. A slight trembling, as from an earthquake, was felt for about half a mile around the workings; and the noise of the explosion, though dull, was heard to three or four miles distance, and much resembled an unsteady fire of infantry. Immense quantities of dust and small coal accompanied these blasts, and rose high into the air, in the form of an inverted cone. The heaviest part of the ejected matter, such as corves, pieces of wood, and small coal, fell near the pits; but the dust, borne away by a strong west wind, fell in a continued shower from the



Engraved by J. Shury, for Dr. Thomsons Annals, Published by R. Baldwin, Paternoster Row, May 1, 1813.

pit to the distance of a mile and a half. In the village of Heworth, it caused a darkness like that of early twilight, and covered the roads so thickly, that the footsteps of passengers were strongly imprinted in it. The heads of both the shaft-frames were blown off, their sides set on fire, and their pullies shattered in pieces; but the pullies of the John Pit gin, being on a crane not within the influence of the blast, were fortunately preserved. The coal dust, ejected from the William Pit into the drift or horizontal parts of the tube, was about three inches thick, and soon burnt to a light cinder. Pieces of burning coal, driven off the solid stratum of the mine, were also blown up this shaft.

As soon as the explosion was heard, the wives and children of the workmen ran to the working-pit. Wildness and terror were pictured in every countenance. The crowd from all sides soon collected to the number of several hundreds, some crying out for a husband, others for a parent or a son, and all deeply affected with an admixture of horror, anxiety, and grief.

The machine being rendered useless by the eruption, the rope of the gin was sent down the pit with all expedition. In the absence of horses, a number of men, whom the wish to be instrumental in rescuing their neighbours from their perilous situation, seemed to supply with strength proportionate to the urgency of the occasion, put their shoulders to the starts or shafts of the gin, and wrought it with astonishing expedition. By twelve o'clock, 32 persons, all that survived this dreadful calamity, were brought to day-light. The dead bodies of two boys, numbers 1 and 4,* who were miserably scorched and shattered, were also brought up at this time: three boys, viz. numbers 2, 3, and 5, out of the 32 who escaped alive, died within a few hours after the accident. Only 29 persons were, therefore, left to relate what they observed of the appearances and effects of this subterraneous thundering: 121 were in the mine when it happened, and 87 remained in the workings. One overman, two wastemen, two deputies, one headsman or putter (who had a violent tooth-ache), and two masons, in all *eight persons*, came up at different intervals, a short time before the explosion.

They who had their friends restored, hastened with them from the dismal scene, and seemed for a while to suffer as much from the excess of joy as they had lately done from grief; and they who were yet held in doubt concerning the fate of their relations and friends, filled the air with shrieks and howlings; went about wringing their hands; and threw their bodies into the most frantic and extravagant gestures.

The persons who now remained in the mine, had all been

* The numbers refer to the situations of the sufferers in the plate.

employed in the workings to which the plane-board was the general avenue, and as none had escaped by that way, the apprehension for their safety began to strengthen every moment. At a quarter after twelve o'clock, Mr. Straker, Mr. Anderson, William Haswell, Edward Rogers, John Wilson, Joseph Pearson, Henry Anderson, Michael Menham, and Joseph Greener, therefore descended the John Pit, in expectation of meeting with some of them alive. As the fire-damp would have instantly ignited at candles, they lighted their way by *steel-mills*, small machines which give light by turning a plain thin cylinder of steel against a piece of flint. Knowing that a great number of the workmen would be at the crane when the explosion happened, they attempted to reach it by the plane-board: but their progress was intercepted at the second pillar by the prevalence of choak-damp: the noxious fluid filled the board between the roof and the thill; and the sparks from the steel fell into it like dark drops of blood. Being, therefore, deprived of light, and nearly poisoned for want of atmospheric air, they retraced their steps to the shaft, and with similar success attempted to pass up the narrow boards: in these they were stopped at the sixth pillar by a thick smoke, which stood like a wall the whole height of the board. Here their flint-mills were not only rendered useless, and respiration became extremely difficult, but the probability of their ever reaching the places where they expected to meet with those they were in search of, or of finding any of them alive, was entirely done away. To the hopelessness of success in their enterprize should also be added, their certainty of the mine being on fire, and the probability of a second explosion at every moment occurring and burying them in its ruins.

At two o'clock Mr. Straker and Mr. Anderson had just ascended the John Pit, and were gone to examine the appearance of the air issuing from the William Pit. Menham, Greener, and Rogers, had also ascended. Two of the party were at this moment in the shaft, and the other two remained below, when a second explosion, much less severe than the first, excited more frightful expressions of grief and terror amongst the relatives of the persons still in the mine. Rogers and Wilson, the persons in the shaft, experienced little inconvenience by the eruption: they felt an unusual heat, but it had no effect in lifting up their bodies, or otherwise destroying the uniformity of the motion of their ascent. Haswell and H. Anderson, hearing its distant growlings, laid themselves down at full length on their faces, and in this posture, by keeping firm hold of a strong wooden prop, placed near the shaft, to support the roof of the mine, experienced no other inconvenience from the blast, than its lifting up their legs and poising their bodies in various directions, in the manner that the waves heave and toss a buoy at sea. As soon as the atmospheric current returned down the shaft, they were drawn to bank.

This expedient of lying down and suffering the fury of the blast to roll over them, is mentioned in the Life of Lord Keeper North, under the year 1676. It is most efficacious where the mine is wet, for atmospheric air always accompanies running water; but the warning of a blast being usually sudden, it requires a degree of experience and coolness, not commonly united, to exercise any precaution against it. The miner knowing its irresistible power, instantly sees the inefficacy of every attempt to escape, and, like a physician attacked by some incurable complaint, and, conscious that his art is unequal to its cure, makes no struggle to save his life.

As each of the party came up, he was surrounded by a group of anxious inquirers. All their reports were equally hopeless; and the second explosion so strongly corroborated their account of the impure state of the mine, that their assertions for the present seemed to be credited. But this impression was only momentary. On recollection, they remembered that persons had survived similar accidents, and when the mine was opened, been found alive. Three had been shut up during 40 days in a pit near Byker, and all that period had subsisted on candles and horse beans. Persons, too, were not wanting to infect the minds of the relatives of the sufferers with disbelief in the accounts of the persons who had explored the mine. It was suggested to them, that want of courage, or bribery, might be inducements to magnify the danger, and represent the impossibility of reaching the bodies of the unfortunate men. By this species of wicked industry, the grief of the neighbourhood began to assume an irritable and gloomy aspect. The proposition to exclude the atmospheric air from the mine, in order to extinguish the fire, was therefore received with the cries of "Murder," and with determinations of opposing the proceeding.

Many of the widows continued about the mouth of the John Pit during the whole of Monday night, with the hope of hearing the voice of a husband or a son calling for assistance.

On Tuesday the 26th of May, the natural propension of the human mind to be gratified with spectacles of horror was strongly exemplified. An immense crowd of colliers from various parts, but especially from the banks of the river Wear, assembled round the pits, and were profuse in reproaches on the persons concerned in the mine, for want of exertion to recover the men. Every one had some example to relate of successful attempts in cases of this kind,—all were large in their professions of readiness to give assistance; but none were found to enter the inflammable jaws of the mine. Their reasonings and assertions seemed indeed to be a mixture of those prejudices and conceits which cleave to workmen whom experience has afforded a partial insight into the nature and peculiarities of their profession, and not to be grounded on any memory of facts, or to result from a knowledge of the connection between causes and effects: and on this account, as soon as the leaders of the outcry could be brought to listen with patience to a relation of the appearances that attended this accident, and to hear the reasons assigned for the conclusion that the mine was on fire, and that the persons remaining in it were dead, they seemed to allow the impracticability of reaching the bodies of the sufferers, till the fire was extinguished, and consequently the necessity of smothering it out by excluding atmospheric air from the mine.

The proprietors of the mine gave the strongest assurances to the crowd, that if any project could be framed for the recovery of the men, no expense should be spared in executing it; if any person could be found to enter the mine, every facility and help should be afforded him; but, as they were assured by the unanimous opinion of several of the most eminent viewers in the neighbourhood, that the workings of the mine were in an unapproachable state, they would hold out no reward for the attempt: they would be accessary to no man's death by persuasion or a bribe.

The mouth of the John Pit had continued open since the accident: the William Pit was to-day almost wholly muzzled with planks.

On Wednesday the 27th of May, at the clamorous solicitation of the people, Mr. Straker and the overman again descended the John Pit, in order to ascertain the state of the air in the workings. Immediately under the shaft they found a mangled horse, in which they supposed they perceived some signs of life; but they had only advanced about six or eight yards, before the sparks of the flint were extinguished in the choak-damp, and Haswell, who played the mill, began to show the effects of the carbonic poison, by faultering in his steps. Mr. Straker therefore laid hold of him, and supported him to the shaft. As the baneful vapours had now taken possession of the whole of the mine, and they found it difficult to breathe even in the course of the full current of the atmospheric air, they immediately ascended. But the afflicted creatures, still clinging to hope, disbelieved their report. Wishful, therefore, to give as ample satisfaction as possible to the unhappy women, Mr. Anderson and James Turnbull (a hewer of the colliery, who had escaped the blast) again went down. At 30 fathoms from the bottom they found the air exceedingly warm: to exist without apoplectic symptoms for more than a few yards round the bottom of the shaft, was found impossible, and even there the air was so contaminated, as to be nearly irrespirable. When they ascended, their clothes emitted a smell somewhat resembling the waters of Gilsland and Harrowgate, but more particularly allied to that of the turpentine distilled from coal tar.

The report of these last adventurers partly succeeded in convincing the people that there was no possibility of any of their friends being found alive. Some, indeed, went away silent, but not satisfied; others with pitiable importunity besought that measures to recover their friends might even yet be adopted and persevered in; and many, as if grief and rage had some necessary connection, went about loading the conductors of the mine with execrations, and threatening revenge. Some were even heard to say they could have borne their loss with fortitude had none of the workmen survived the calamity: they could have been consoled had all their neighbours been rendered as miserable and destitute as themselves! From such a multitude of distracted women, unanimity of sentiment could not be expected—no scheme of proceedings could be invented fortunate enough to meet with the approbation of them all. In the evening of this day it was, therefore, resolved to exclude the atmospheric air from entering the workings, in order to extinguish the fire which the explosion had kindled in the mine, and of which the smoke ascending the William Pit was a sure indication. This shaft was accordingly filled with clay about seven feet above the *ingate* or entrance from the shaft into the drift; and the John Pit mouth was covered over with loose planks.

On Thursday the 28th of May, both the pits continued in the state they were left in on the preceding evening; but early on the morning of the 29th, 20 fothers of additional thickness in clay were thrown into the William Pit, in order to insure its being air tight; and on the same day, a scaffold, at 25 fathoms and a half from the surface, was suspended on six ropes, each six inches in circumference, in the John Pit. Upon this, ten folds of straw were thrown, and 26 fothers of clay; namely, 15 fothers on Friday, 5 on Saturday, and 6 on Sunday; on which day the scaffold was found sufficiently air tight, by its holding the water poured upon it.

(To be concluded in our next.)

Article VI.

An Account of a Salt composed of Sulphate of Soda, Muriate of Manganese, and Muriate of Lead: By Mr. Daniel Wilson, Dublin.

For the purpose of making oxymuriate of lime, three parts of muriate of soda, and one part of finely ground black oxide of manganese, are well mixed, and put into a cast-iron still with a leaden cover, connected by a pipe of the same metal with a wooden base, or chamber, in which there is thinly spread one and a half parts of dry lime, that has been newly slaked and sifted. About four parts of sulphuric acid, of the specific gravity 1·500, are added at intervals through a funnel in the cover of the still; this causes an immediate extrication of oxymuriatic acid gas, which passes into the chamber, and combines with the lime. When the disengagement of gas becomes languid, a fire is applied, and the matter within is well stirred by means of an agitation that passes through the cover; the heat is now gradually increased, until the gas ceases to pass over, when a plug is drawn from the bottom of the still, which allows its contents to flow out in a half liquid state.

When this substance remains at rest for a few hours, a dense, black, deliquescent matter subsides, and a greenish yellow coloured liquor floats on the top, and continues to ooze out from the more solid part. The black deliquescent substance is in commerce called Bleacher's Residuum; it is composed of variable proportions of sulphate of soda, muriate of soda, muriate of manganese, muriate of iron, muriate of lead, oxides of manganese and iron, silex, alumin, and a small quantity of uncombined muriatic and oxymuriatic acids.

The greenish yellow part is called sour liquor; it varies in specific gravity from 1·350 to 1·450, and is composed of muriatic and oxymuriatic acids, sulphate of soda, muriate of soda, muriate of manganese, muriate of lead, and muriate of iron.

Several vitriol bottles having been filled with this sour liquor, and exposed for some weeks to a temperature that varied from 30° to 45° Fahr., a salt slowly formed in groupes of hard crystals firmly adhering to each other, which possesses the following properties:

1. It is of a brownish yellow colour, and a disagreeable metallic taste. 2. It crystallizes in octahedrons, which are sometimes composed of two four-sided pyramids united by their bases, but are oftener irregular. 3. Its specific gravity is 2·390. 4. It deliquesces in a moist, and effloresces in a dry, atmosphere. 5. When deprived of its acid mother water by being washed, it does not change the colour of vegetable blues. 6. It is soluble in 2·5 times its weight of water at the temperature of 60°, and in ·75 its weight of boiling water. 7. With prussiate of potash and iron it forms a yellowish white precipitate. 8. A small quantity of a brown coloured precipitate is slowly deposited, on its solution being dropped into an infusion of nutgalls. 9. Fixed and volatile alkalies form white precipitates with its solution, which soon turn brown when exposed to the atmosphere. 10. When exposed to heat, it loses its water of crystallization without undergoing the watery fusion; at a low red heat it

melts, and gives out muriatic acid, if the temperature be increased. 11. It is decomposed by solution in water; muriate of lead precipitates; sulphate of soda crystallizes on cooling; and muriate of manganese remains in the mother water. 12. When digested with sulphuric acid, copious fumes of muriatic acid gas are disengaged.

Having ascertained by a number of experiments that this salt is composed of sulphate of soda, muriate of manganese, and a small quantity of muriate of lead, the following were made to discover the proportions of each:—

I. One hundred grains of this salt added to a quantity of water at the temperature of 60° quickly dissolved, except a small portion of a dense white matter which remained at the bottom of the vessel, and a few particles of a brown substance that more slowly subsided. The white matter was washed with cold water, and exposed to a low red heat, when it fused; in this state it weighed 1·5 grain. It was found to be soluble in a large quantity of boiling water, which on cooling deposited minute crystals of a pearly lustre; sulphate of silver added to a solution of these crystals, occasioned a white fleaky precipitate; sulphate of soda deposited a heavy white powder; and the alkalies precipitated a white substance, which, on exposure to a red heat with charcoal, yielded a globule of metal resembling lead. The white insoluble matter is therefore muriate of lead, and, according to the analysis of Kirwan, is composed of

·25	acid	1·5
1·25	oxide	

The light brown precipitate, when collected and exposed to a red heat, weighed ·5 grain.

On a larger quantity being procured, it was found to be soluble in muriatic acid; the solution yielded a yellowish white precipitate, with triple prussiate of potash; and the alkalies deposited a white substance, which quickly turned brown on exposure to the atmosphere, or on being heated; properties belonging to the oxide of manganese.

II. One hundred grains of this salt were dissolved in water, and the solution separated from the insoluble matters; muriate of barytes was added to the solution as long as any precipitate continued to be formed; the precipitate was then well washed and ignited; it weighed 93 grains, which indicate 54·7 grains of sulphate of soda, that are, according to Kirwan, composed of

30·63	acid	54·7
24·07	alkali	

III. Sulphate of silver was added to a solution of 100 grains of this salt until it ceased to form a precipitate; the edulcorated muriate of silver was then collected and fused; it weighed 63 grains, which indicate 10·42 of muriatic acid.

IV. A solution of soda was added to 100 grains of this salt dissolved in water as long as it continued to occasion any precipitate; the precipitate was then well washed, and exposed to a red heat; it weighed 16 grains, and was found to be pure oxide of manganese.

V. One hundred grains of this salt were exposed to heat in an iron crucible; at a low red heat it began to melt and boil; when the temperature had increased to a bright red, the boiling ceased, and the whole remained in the state of a thin, transparent fluid; when weighed in this state it was found to have lost 16 grains, which may be considered the quantity of water that it contains.

VI. One hundred grains of this salt were dissolved in water; the solution separated from the insoluble substances, and evaporated to saturation; a quantity of alcohol, equal to four times its bulk, was then added to it, and the mixture agitated; sulphate of soda quickly precipitated, and the muriate of manganese remained dissolved by the alcohol. The precipitate was then separated from the solution, treated several times with small quantities of alcohol, for the purpose of dissolving any muriate that might adhere to it, and exposed to a red heat, when it weighed 55 grains. The alcohol was then evaporated, and the residual salt fused; it weighed 26·5 grains. The salts thus procured were dissolved in water, and tested by the muriate of barytes and sulphate of silver; the separation was not perfectly complete; the sulphate of barytes obtained denoted very nearly the same quantity of sulphate of soda that the muriate of silver did of muriate of manganese; but in neither case did the precipitates indicate a grain of the respective salts. The results obtained by this experiment agree very nearly with those of the preceding: this salt may therefore be considered as composed of

30·63	Sulphuric acid.
10·67	Muriatic acid.
24·07	Soda.
16·5	Oxide of manganese.
1·25	Oxide of lead.
16·	Water.
·88	Loss.
100·00	

I have found the quantity of uncombined manganese remarked in the first experiment to vary in proportion to the purity of the salt; the weight given there may be considered as rather more than average of several; it is therefore probable that its presence is accidental; but with regard to the muriate of lead, I am inclined to suppose it a necessary constituent of the salt, from observing the uniform quantity in which it is produced, however regular in their shape and transparent the crystals may be. From the peculiar nature of the salt, it is difficult to verify this conjecture by experiment; the affinities which its constituents exert on each other are so easily disturbed, that solution in water is sufficient to cause their separation: when acid solvents are used the same effect is produced; in all cases the muriate of lead is rather decomposed, or precipitated in a half crystallized state, sulphate of soda crystallizes uncombined, in a quantity which exceeds in weight the whole of the salt dissolved, and muriate of manganese remains in the mother water. I have not succeeded in any instance in reproducing the compound.

Until more successful experiments enable us to decide upon this subject with a greater degree of certainty, it may perhaps be better to consider the muriate of lead a necessary part of the salt, and the uncombined manganese to be accidentally present: 100 parts of it, then, are composed of very nearly

55·47	Sulphate of soda.
26·79	Muriate of manganese.
1·52	Muriate of lead.
16·22	Water.
100·00	

The action of the acid gases on the cover, and on the agitator of the still, is the cause of the formation of the muriate of lead; but a difficulty still remained to be accounted for, as, according to the received affinities, sulphate of soda and muriate of lead are incapable of existing together in the same liquor. The following are a few out of a number of experiments made on the subject:—

1. Muriate of lead, boiled in a solution of sulphate of soda, was converted into sulphate of lead; and no crystals of the muriate of lead were obtained on cooling the solution.

2. Muriate of lead and sulphate of soda, boiled together in diluted muriatic acid, were wholly dissolved, and yielded on cooling crystals of muriate of lead; no sulphate of lead appeared to be formed.

3. Muriate of lead, boiled in *sour liquor*, was quickly dissolved, and crystallized abundantly on cooling.

4. Sulphate of soda, muriate of manganese, and muriate of lead, boiled together, yielded on cooling no crystals of muriate of lead.

5. Muriate of lead, boiled in diluted sulphuric acid, was

converted into sulphate, and no crystals appeared on cooling the liquor.

6. Sulphate of lead, boiled in diluted muriatic acid, was decomposed and dissolved; crystals of muriate of lead formed on cooling the solution.

Sulphate of soda, therefore, does not decompose muriate of lead when an excess of muriatic acid is present, and sulphate of lead is decomposed by muriatic acid, and muriate of lead by sulphuric acid, a case of affinities that cannot easily be explained.

I have been thus particular in describing the properties of this substance, from the circumstance of its being the only instance, with which I am acquainted, of a salt composed of different acids united to different bases; which peculiarity, if found to extend to a greater number of bodies, may throw some additional light on our knowledge of affinity.

Since the investigations of chemists have made known the nature and properties of triple salts, some of the most obscure parts of practical chemistry have been rendered comparatively plain; but there are yet several circumstances connected with the salts that cannot satisfactorily be explained; and until their affinities are more accurately ascertained, by the discovery and examination of a greater number of substances composed of different genera of salts, we can have but a partial knowledge of the influence which some of them have in preventing the crystallization of others, and of the true reason why certain saline bodies are more soluble in the solutions of others than in water alone.

Article VII.

Astronomical Observations at Hackney Wick. By Col. Beaufoy.

Latitude 51° 32′ 40″ North. Longitude West in Time 6·82″.

1813.

	Time at H. W.			Time at Greenwich.			
March 26, Emersion of Jupiter's 1st satellite	10h	43′	52″	10h	43′	58″	82‴
April 11, Ditto, Ditto, 1st Ditto	9	02	06	9	02	12	82
April 13, Commencement of the egress of Jupiter's 4th satellite from his disc	9	57	53	9	57	59	82
April 13, Emersion of Jupiter's 2d satellite	10	18	05	10	18	11	82
April 17, Immersion of γ Libræ...... (N. B. The Moon's Limb was very tremulous.)	10	24	28	—	—	—	—

N. B. The Observations are in Mean Time.

Rain between March the 1st and 31st, 0·662 inches.

Observations on the Variation of the Needle.

Month.	Morning Observ.					Noon Observ.					Evening Observ.					
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.			
March 30	9h	30′	24°	10′	29″	12h	23′	24°	24′	16″	2h	15′	24°	19′	51″	
Ditto 31	8	37	24	9	12	12	20	24	24	12	2	50	24	21	42	
April 2	8	50	24	9	18	1	50	24	24	25	5	05	24	11	28	
Ditto 3	8	20	24	5	34	1	00	24	25	09	—	—	—	—	—	
Ditto 4	—	—	—	—	—	12	35	24	21	30	—	—	—	—	—	
Ditto 5	7	30	24	6	30	—	—	—	—	—	—	—	—	—	—	
Ditto 6	—	—	—	—	—	1	00	24	24	18	5	30	24	13	14	
Ditto 7	9	30	24	9	10	1	00	24	11	58	3	10	24	18	23	*
Ditto 8	9	00	24	7	04	—	—	—	—	—	—	—	—	—	—	
Ditto 9	8	30	24	11	00	12	35	24	25	19	—	—	—	—	—	
Ditto 10	6	35	24	12	17	—	—	—	—	—	5	15	24	17	44	
Ditto 11	8	03	24	10	19	12	25	24	18	33	9	30	24	11	16	
Ditto 12	8	05	24	9	37	12	30	24	18	22	5	30	24	10	42	
Ditto 13	7	10	24	7	20	12	45	24	22	02	5	30	24	12	50	
Ditto 14	9	00	24	10	53	1	05	24	20	27	5	40	24	19	57	
Ditto 15	9	40	24	10	50	1	20	24	20	23	4	20	24	12	32	
Ditto 16	8	50	24	10	26	1	30	24	17	51	7	20	24	15	6	
Mean	9	9	24	9	20	12	53	24	21	20	5	10	24	15	24	

* Wind S.E. to S.W. and hazy.

I can assign no cause why the Noon Observation on the 7th is so much less than the others.

Article VIII.

Analyses of Books.

Elements of Chemical Philosophy. By Sir Humphry Davy, LL.D. Sec. R.S. Prof. Chem. R.I. and B.A. M.R.I. F.R.S.E. M.R.I.A. Member of the Royal Academy of Stockholm; of the Imperial Med. and Chirur. Academy of St. Petersburgh; of the American Philosophical Society; and Honorary Member of the Societiés of Dublin and Manchester, the Physical Society of Edinburgh, and the Medical Society of London. Part. I. Vol. I. London. Johnson and Co. 1812, 8vo. pages 511.

The celebrated author of this work begins with an introduction, in which he gives a very comprehensive and entertaining account of the history of chemistry, from its first rude dawn among the Arabians down to our own times. This introduction is entitled to almost unlimited praise, both on account of the very extensive knowledge of the subject which the author displays, and for the candour with which he speaks of the labours of his predecessors. A severe critic, indeed, might point out a few trifling mistakes into which he has fallen; but they are of little or no consequence, and do not detract in the least from the merit of the whole.

It is obvious that this volume is only a small part of a great whole; which, doubtless, is nothing less than a complete system of chemistry. Such a work, when completed, cannot occupy fewer than five or six volumes, of the same size as the present; but I own I entertain considerable doubts whether the work will ever be completed. No person entertains a higher opinion than myself of the talents and industry of Sir Humphry Davy; but to publish a complete system of chemistry, all the facts contained in which shall have been verified by the author, I hold to be impossible. A very considerable part must of necessity be founded on compilation, and after what has already been done in that way by others, Sir Humphry Davy could hardly execute his task in such a manner as not to diminish his reputation. The volume before us will serve as a corroboration of this. It is totally occupied with those substances to which Sir Humphry Davy has devoted the greatest part of his attention, and to his discoveries respecting which his reputation is entirely owing. Yet even in this volume there is a very long and important article, I mean *the account of heat*, which is nothing else than a compilation. I do not blame Sir H. Davy for this: it was out of his power to have acted otherwise, without omitting the subject altogether; but it is quite obvious that if the volume before us had contained nothing more original than this, the work would have detracted from, instead of adding to, his reputation. Now this can scarcely avoid being the case with some of the subsequent volumes.

The title of the portion of chemistry detailed in this volume is, Part. I. *On the laws of chemical changes: on undecompounded bodies, and their primary combinations.* It would have been better to have subdivided this division into two distinct parts; for it treats of two distinct subjects, that cannot be well amalgamated together; namely, 1. The general laws of chemistry. 2. A description of the simple substances, and their primary compounds. Arrangement may appear at first sight an object of secondary importance; but, in reality, it is one of the very first magnitude, because upon it the perspicuity of the whole entirely depends. If a system of chemistry be ill arranged, whatever be its merits in other respects, it never can become popular, because it will not be understood by beginners, on whom the sale of such works chiefly depends. Chemical writers, in general, do not seem to have sufficiently appreciated the importance of systematic arrangement. When I first published my System of Chemistry there was no better arrangement than that of Macquer, Gren, and Chaptal, which were by no means fitted to the state of the science. My arrangement was quite different, and totally new, and constituted in fact the great difficulty of constructing such a system, because it obliged me to discard all preceding systematic books, and to construct my system out of the original materials furnished by the chemical discoverers themselves. This arrangement was disapproved of *in toto* by every person who reviewed the work, both in Britain and France; yet it has been adopted (with a few modifications) by every subsequent writer on the subject, and even at this moment continues better adapted to the present state of the science than any other which I have yet seen.

This Part I. which constitutes the volume, is split into seven subordinate heads, called by the author *divisions*. Let us take a view of each of these in order.

Division I. *On the powers and forms of matter, and the general laws of chemical changes.* In this division he gives an account of the three different forms of matter, namely, solidity, liquidity, and elastic fluidity; of gravitation; of cohesion; and of heat. Of the phenomena of heat he gives a pretty clear, though concise account, and concludes with giving it as his opinion that heat is nothing else than motion, and that the laws of heat are the same as the laws of motion. There have been always two opinions respecting heat, which have divided philosophers. According to one party, heat, like gravitation, is merely a property of matter; according to the other, it is a peculiar substance. Both of these opinions may be supported with considerable plausibility. Some of the advocates for the first opinion have endeavoured, like Sir Humphry Davy, to go a step farther, and to show how the phenomena of heat are produced by motion; but in my opinion their conduct has been injudicious. It is easy to defend the opinion that heat is a mere property of matter; and in the present state of our knowledge, impossible to refute it: but when a philosopher proceeds to explain by what kind of motion heat is produced, he loses all the advantages that attended the general opinion; as nothing is easier than to demonstrate the insufficiency of any kind of motion hitherto devised, from that of Boyle and Mayow down to that of Davy, to produce the phenomena. When it is said that the laws of the communication of heat are the same as the laws of the communication of motion, I confess that for my part I am at a loss to comprehend the meaning of the assertion. So far from conceiving them to be the same, I can see no resemblance between them, except that a body, by communicating temperature to another, loses temperature itself, just as a body by communicating motion to another loses some of its own velocity.

The opinion that heat is a body is so consonant to the common sense of mankind, that I defy any man to explain the phenomena of heat without using a language which implies it, how hostile soever his own notions may be to this opinion. In this respect it agrees with the Newtonian doctrine of light. As the opinion that heat is a body was universally embraced by chemical philosophers for about 40 years, it is not surprising that the love of novelty, or the discovery of new facts, should induce modern philosophers to incline to the contrary opinion. This vacillation I conceive to be of great service to the knowledge of the subject, by inducing the pioneers of science to neglect no part of the subject, but to break up the ground in every quarter, and thus to throw together a much greater mass of important facts than could otherwise be collected. But the knowledge of the subject is not yet far enough advanced to bear the conclusions of Count Rumford and Sir Humphry Davy; perhaps it may never reach such a point, for some subjects are so much beyond the reach of the human faculties that the more they are discussed the more obscure they become.

Sir Humphry Davy next takes a general view of the phenomena of chemical affinity. This part of the work is chiefly calculated for those who have made some progress in chemistry; and will be read with most interest by those who are best acquainted with the science. The following are the general heads stripped of all their illustrations. Some bodies, as oil and water, do not unite together; while others, as salt and water, do. When substances combine, the qualities of the new compound differ very much from those of its constituents. Bodies before they unite chemically must be in apparent contact; but it is not necessary that one of them be in a state of liquidity. Substances vary in the intensity of their affinity. Hence one substance is capable of displacing another, and these decompositions are facilitated by the mutual action of four bodies, or what are called double decompositions. All chemical compounds consist of bodies united in definite and constant proportions. Sir H. Davy is happy in the numerous illustrations which he has adduced in proof of this fact. The most beautiful are those of the combinations of the gases. The respective weights of bodies that unite may be represented by numbers and their multiples, and the following are the numbers belonging to the elementary bodies which he notices in this part of his work:—

Hydrogen	1
Oxygen	15
Azote	26
Chlorine	67
Potassium	75

These numbers would require some animadversions; but the subject could not be understood without entering into much longer details than would be consistent with this article. I propose publishing an essay on it in a subsequent number of this journal. Mere convenience is the only reason assigned by the author for pitching upon 15 to represent oxygen. If we were to be directed by convenience alone there could be no hesitation in choosing 10, the number proposed by Dr. Wollaston, which would afford more arithmetical facility than any other number. But I conceive that a better basis may be obtained for a groundwork than mere facility; and in fact the difficulties which such elementary arithmetical processes present are of too slight a nature to claim much attention.

Sir Humphry Davy terminates this part of the subject with an examination of the peculiar opinions respecting affinity which have been supported by Berthollet. That Berthollet in some of his notions has gone much farther than he can be borne out by facts is, I think, incontrovertible. Thus when he affirms that substances are capable of combining in any proportion whatever, he is refuted by all the chemical compounds which have been hitherto examined, excepting aqueous solutions and alloys, which are more similar to mixtures than chemical compounds. When he affirms that substances divide another between them, according to their rate of affinity for it, and that bodies are seldom or never thrown down in a state of absolute purity; his opinion is opposed by the knowledge of the determinate proportions in which bodies combine, and by the permanency and comparatively small number of chemical combinations; but to condemn all his opinions in the lump, appears to me to be going just as far wrong on the one side as he has done on the other. That he has succeeded in demonstrating the inaccuracy of many of the old notions respecting affinity, appears to me incontrovertible; though he has not been so fortunate in establishing his own. Nor are we at present in possession of any precise notions respecting the strength of the affinity which different bodies have for each other.

Next follows a very neat and distinct enumeration of the phenomena of electricity, of considerable value, especially that part of it which treats of galvanism. Several important general laws are stated, some of them not hitherto attended to by electricians; but unluckily the whole subject is treated so briefly as to preclude the possibility of giving any sketch of it. This brevity, I should think, will be felt by a beginner, and will give an air of obscurity to this very valuable section.

This part of the work is concluded with some very judicious observations on Analysis and Synthesis, on the mode of experimenting, and upon the state in which vapour exists in gases.

In the second division Sir Humphry Davy treats of what he calls *radiant or ethereal matter.* He considers it as producing light, as producing heat; and enters into some refined speculations respecting the conversion of terrestrial bodies into light, and vice versa; speculations which I avoid examining, because they do not admit either of proof or refutation; and though highly ingenious, treat, I fear, of subjects which will ever remain beyond the reach of the human faculties.

The third division is dedicated to the description of undecompounded supporters of combustion. These are two, oxygen gas and chlorine or oxymuriatic acid. The description of the properties of both is minute, and, as far as I can judge, very accurate; but not susceptible, from their very nature of abridgment.

The fourth division treats of the simple combustible substances that are not metals, and of their combinations with oxygen, with chlorine, and with each other. These substances are, hydrogen, azote, sulphur, phosphorus, carbon and diamond, and boron. The following table exhibits the compounds of these substances with oxygen:—

			Oxygen.			
Hydrogen	1	+	7·5	=	8·5	water.
Azote	13	+	7·5	=	20·5	nitrous oxide.
	6·5	+	7·5	=	14	nitrous oxide.
	6·5	+	15	=	21·5	nitrous acid.
	6·5	+	22·5	=	29	nitric acid.
Sulphur	15	+	15	=	30	sulphurous acid.
	15	+	22·5	=	37·5	sulphuric acid.
Phosphorus	10	+	7·5	=	17·5	phosphorous acid.
	10	+	15	=	25	phosphoric acid.
	20	+	7·5	=	27·5	oxide of phosphorus.
Carbon	5·7	+	15	=	20·7	carbonic acid.
	5·7	+	7·5	=	13·2	carbonic oxide.
Boron	——	+	——	=	——	boracic acid.

The following table exhibits the combinations which these bodies form with chlorine.

			Chlorine.			
Hydrogen	1	+	33·5	=	34·5	muriatic acid.
Azote	Not determined.					
Sulphur	15	+	67	=	82	sulphurane.
Phosphorus	10	+	33·5	=	43·5	phosphorane.
	10	+	67	=	77	phosphorana.
Carbon	Does not combine.					
Boron	Unknown.					

The fifth division treats of the metals in the following order:

1. Potassium.	20. Cobalt.
2. Sodium.	21. Copper.
3. Borium.	22. Nickel.
4. Strontium.	23. Uranium.
5. Calcium.	24. Osmium.
6. Magnesium.	25. Tungsten.
7. Aluminum.	26. Titanium.
8. Glucinum.	27. Columbium.
9. Zirconium.	28. Cerium.
10. Silicum.	29. Palladium.
11. Yttrium.	30. Iridium.
12. Manganese.	31. Rhodium.
13. Zinc.	32. Mercury.
14. Tin.	33. Silver.
15. Iron.	34. Gold.
16. Lead.	35. Platinum.
17. Antimony.	36. Arsenic.
18. Bismuth.	37. Molybdenum.
19. Tellurium.	38. Chromium.

In the sixth division he treats of the fluoric acid, and of the amalgam procured by the action of galvanism on mercury, in contact with sal-ammoniac.

In the seventh division we have several ingenious speculations respecting the probable constitution of those substances which have not hitherto been decompounded. These speculations may be of considerable service in directing the investigations of future experimenters; but we see from the example of Lavoisier how much risk every person runs of mistakes who ventures to indulge in such speculations.

Article IX.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Explosion at Woolwich.*

Having been requested by different correspondents to give some account of the dreadful explosion which happened some time ago at Woolwich, we have made some inquiries on the subject; and the following are the facts, as far as we could learn them. A very large room was filled with wood destined for ship-building; and by various obvious contrivances it was kept constantly heated to a temperature rather higher than 120°. The mouth of a large apparatus for distilling coal entered into this room, so that it was kept constantly filled with the vapour of coal tar, and carbureted hydrogen gas. By the heat of the room the water was slowly expelled from the wood, and the empyreumatic oil from the coal took its place. By this contrivance the wood was not only thoroughly dried, but was prevented from again imbibing water by being soked with oil. It is obvious that the air of the room would be a mixture of the inflammable gas from oil and the common air. Now we learn from Dr. Henry's experiments that the medium specific gravity of the gas from coal is scarcely equal to $\frac{1}{2}$ that of common air. For complete combustion it requires scarcely so much as twice its bulk of oxygen gas. The result of my experiments was, that it would not burn unless it amounted to rather more than $\frac{1}{12}$th of the common air with which it was mixed. For complete combustion it would require about nine times its bulk of common air; but I believe that complete combustion never takes place in such mixtures.

These facts are sufficient to account for the explosion at Woolwich. We have only to suppose that the inflammable gas in the room exceeded $\frac{1}{12}$th of the common air. There was a flue that ran along the floor of the room. Somehow or other the flame must have issued through this flue at the moment the damper was applied at the top of the building; for the explosion took place just at that instant. The first combustion would be imperfect; more common air would rush in immediately after the first explosion; and this new mixture, kindled in the same way as the first, produced the second explosion. It is needless to say that the house was completely demolished. Nine men were unfortunately killed. The explosion was precisely similar to what happens so frequently in coal-mines; a very dismal example of which will be found in a preceding part of this number of our Annals.

II. *New Properties of Light.*

In our last number we gave an account of some of the curious discoveries respecting light lately made by the French philosophers, and by Dr. Brewster in this country. We shall now finish what we have farther to say on that subject. Dr. Brewster confirmed Sir Isaac Newton's conjecture that the colours produced by heat upon the surface of polished steel are owing to a thin glassy transparent film of oxide, of various thickness, cooling the surface of the steel; for he found the light reflected from this surface at a certain angle polarized, while the light from the surface of the steel itself was not polarized. Dr. Brewster found, contrary to the assertion of Malus, that light suffers some modification by reflection from metallic surfaces. He conceives that part of it is polarized, and that part of it remains unaltered. We have only to add, that a full account of these interesting discoveries will be found in Dr. Brewster's treatise on new philosophical instruments, just published.

III. *Fungin.*

This name has been recently given by Braconnot to the fleshy part of mushrooms, which he conceives to be a peculiar vegetable principle; and which, according to him, possesses the following properties :—

It may be obtained pure by boiling it in a weak alkaline solution. In that state it is whitish, soft, insipid, possesses little elasticity, and readily yields to the teeth. It would appear that fungin thus purified may be used as an article of food, from what mushroom soever it has been obtained. The poisonous qualities of mushrooms, it would seem, reside in the juices, not in the fungin. This substance, when dried, burns with considerable splendour, emitting an odour similar to that of burning bread, and leaving behind it a white ash.

Dried fungin, when distilled in a retort, yields about half its weight of a liquid product, consisting partly of a brown oil, and partly of water, holding a good deal of ammonia in solution. It yields no acid, which distinguishes it very much from wood. The charcoal remaining in the retort amounts to rather more than one-fourth of the dry fungin subjected to distillation. This charcoal exhibited traces of sulphureted hydrogen, and contained sand, phosphate of lime, and traces of carbonate of lime, phosphate of lime, and of alumina.

Fungin does not dissolve in alkaline solutions, in which respect it differs essentially from lignin, which is readily dissolved by a weak alkali; but if fungin be boiled in a very strong alkaline lie it is partly dissolved, and a saponaceous product obtained. Ammonia dissolves a small portion of fungin, and lets it fall again in white flocks when exposed to the air.

Weak sulphuric acid has no action on fungin; but when concentrated, this acid chars it, and at the same time sulphurous acid and vinegar are formed.

Muriatic acid dissolves it very slowly, and converts it into a gelatinous matter. It is thrown down in flocks by the addition of potash to the acid. Chlorine passed over dry fungin suspended in water converts it into a yellow matter, having at first an acrid taste, which it gradually loses by exposure to the air.

When digested in diluted nitric acid, azotic gas is disengaged. Heated with concentrated nitric acid, it swells, and effervesces, at first violently, but the action soon subsides. When the acid is driven off there remains a liquor, containing oxalate of lime, some prussic acid, and two fatty matters, the most abundant similar to tallow, the other to wax. By evaporating the liquid a considerable quantity of oxalic acid in crystals was obtained. The mother water still contained oxalic acid, and a portion of the bitter principle from indigo.

When fungin is steeped in an infusion of nutgalls, it imbibes a considerable portion of the tannin, and acquires a fawn colour.

When left to putrefy spontaneously in water, it emitted first the odour of putrefying gluten, then that of putrid meat. Neither acid nor ammonia was found in the water; but it contained a portion of mucilage, which gave it viscocity, and the property of precipitating with acetate of lead. The fungin itself assumed the aspect of gluten, without however possessing its properties.

IV. *Extract of a Letter from Dr. John Redman Coxe, Professor of Chemistry at Philadelphia. Dated August* 1, 1812.

I beg leave to mention to you, that authors have generally considered that muriatic acid and alcohol have little action on each other, except through some complex affinities, which it will be unnecessary to state. I have, however, discovered that *time* is an agent of some importance in producing an union, and a consequent formation of ether. In 1809-10 I had mixed equal parts of those substances (in a vial) of the common standards, and had left them corked, occasionally opening the vial, to ascertain what change ensued. For several months the muriatic smell predominated, and I was led to conclude no change would occur; in consequence of which, I laid aside the vial, and did not think of it for upwards of a year: on accidentally looking for something else, I found the vial; and on opening it, was surprised to find a strong ethereal smell, which still continues, although I have frequently since opened the bottle. The muriatic smell is gone; and I mean to try if distillation will not extricate an ethereal fluid. I leave it, however, until winter, in hopes of being more certain than during the hot weather. Not anticipating such a result, the quantities are but small, and of course less likely to evolve much, if any. As the fact may be new to you, I thought it might be acceptable, as it evinces that certain actions may not ensue, until after the expiration of a considerable period.

Another fact I shall call your attention to, is the presumed necessity of atmospheric pressure in certain cases of crystallization. You well know the common explanation of a saturated solution of sulphate of soda tied up warm, and remaining fluid until the bottle is opened, or exposed to such pressure. It is evident, however, that some other explanation must be sought for, since I have ascertained, by repeated trials, that the bottle thus prepared may be set aside uncorked, exposed to the full action of the air, and that it will nevertheless remain perfectly fluid, even if taken up with care, and solidification does not ensue until agitation takes place. I had made a solution of this salt in a large vial to the full extent that the water could take up by exposure to an almost naked fire. At this time I was obliged to leave it, and neglected to cork it, fully expecting the next day to find it completely solid. It was, however, perfectly fluid; but solidified almost instantly on my taking it from the table. Since that period I have annually exhibited to my Class bottles equally strong in solution, some corked, and others uncorked, and both remaining fluid at the end of 24 hours, and becoming solid by the usual means of agitation after the cover of the former was penetrated, and by shaking the latter. I have also found occasionally bottles thus prepared and corked, solidified where I had no reason to expect it; and frequently I have seen *a part* of the salt reduced to a solid form in a close bottle, and when opened and shaken the whole becoming solid, with a singular change in the appearance of that part of the salt which had been previously precipitated. It assumed, whilst the latter precipitation or formation of crystals was going on, a very beautiful porcellanous white appearance, which remained permanent at the bottom of the last formed crystalline mass, and I have now some in that state thus produced during the last winter. It has occurred so frequently, that I cannot consider it accidental; but I am at a loss to explain the fact. From mere appearance I should conclude the salt thus previously produced had entirely given up its water of crystallization. That something more than mere atmospheric pressure is necessary, however, to explain the common occurrence cannot, I think, be denied. I shall be glad to find it succeed in your hands, and to have your opinion respecting it. If the common explanation was correct, why can we not effect the same with nitrate of potash? I have repeatedly tried, but ineffectually.

I have several times, since I wrote to you last, tried the reforming of pyrophorus, by adding a portion of solution of potash to imperfect pyrophorus, and exposing to heat, with complete success. I have, therefore, little doubt of the whole phenomena being dependant on the formation of a portion of potassium spread through the mass, and inflaming through the instrumentality of atmospheric moisture, &c. I have even once tried it successfully on pyrophorus already ignited by exposure to the air; and hence it may be useful to preserve that which has undergone the change, till a sufficient quantity is obtained to be again treated with the addition of potash, since it will save the trouble of the preliminary steps, and we have at hand the materials in a very dry and perfect state for the process.

V. *Use of Magnesia in Calculous Diseases.*

The Editor is requested by Dr. Henry to state, that he never made the least claim to the suggestion of the trial of magnesia in calculous diseases (erroneously ascribed to him in a recent and valuable work of Professor Berzelius on animal chemistry), and that the merit of the hint, which led to the successful experiments of Mr. Brande, belongs entirely to Mr. Hatchett.

VI. *Pepper.*

There is a very singular fraud which has been practised for some time past in some of the retail shops in London. Artificial pepper-corns, both white and black, are mixed with real pepper-corns, and this fraudulent mixture sold as genuine pepper. The mode of detecting the cheat is easy. Throw a handful of the suspected pepper-corns into water: the artificial corns fall to powder, or are partially dissolved; while the true pepper-corns remain whole. I am told that these fraudulent pepper-corns are made of peasmeal. The fraud should be publicly known, because such a mixture, if used instead of real pepper, may prove, in many cases of household economy, exceedingly prejudicial to those who ignorantly make use of it.

Article X.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On the 1st of March some additional observations on the tusks of the narwal, by Sir Everard Home, Bart. were read. He found in the skull of a female narwal two milk tusks, about nine inches long. Hence that animal, when full grown, would have had tusks. On sawing in two the tusk of a narwal it was found mostly hollow. This is the reason why it cannot be used, like ivory, for the purposes of art.

At the same meeting of the Society, a paper by Dr. Wells was read, giving an account of a woman, the offspring of white parents, part of whose skin was black. The name of the woman is Harriet West. She was born in Suffolk, about eight miles from the sea coast; and she is at present about 23 years of age. Her father was a footman, and died when she was very young. She is the only child of her father; but her mother, who was married a second time, has had 11 children since, all white. Her mother, when pregnant with her, got a fright by trampling on a live lobster; and to this the spots on her skin were ascribed. The whole of her body is very white, except the right shoulder, arm, and hand, which are mostly black, except a white stripe on the fore arm. The black parts are darker than in a negro. Winslow has observed, that the cuticle in negroes is black; and

Dr. Wells found this the case with the black cuticle of Harriet West. From this curious case, Dr. Wells draws the following inferences:—1. The black colour of negroes does not prove them to be a distinct race of animals from the whites. 2. The black colour cannot be ascribed to the action of the sun merely, as is the common opinion. An additional proof of the fallacy of such an opinion is, that those parts of negroes which are exposed to the sun are not so black as those that are covered with clothes.

On Thursday the 8th of March Dr. Wells's paper was concluded. He gave his opinion about what occasioned the difference between negroes and whites. It is well known that whites are not so well able to bear a warm climate as negroes; and that they are liable to many diseases in such a situation, from which negroes are free. On the other hand, whites are much better fitted to bear a cold climate than negroes. Suppose a colony of whites transported to the torrid zone, and obliged to subsist by their labour, it is obvious that a great proportion of them would speedily be destroyed by the climate, and the colony, in no long period of time, annihilated. The same thing would happen to a colony of negroes transported to a cold climate. Dr. Wells conceives that the black colour of negroes is not the cause of their being better able to bear a warm climate, but merely the sign of some difference in constitution, which makes them able to bear such a climate. Suppose a colony of white men carried to the torrid zone; some would be better able to resist the climate than others. Such families would thrive, while the others decayed. These families would exhibit the sign of such a constitution; that is, they would be dark: and as the darker they were, the better they would be able to resist the climate; it is obvious that the darker varieties would be the most thriving, and that the colony, on that account, would become gradually darker and darker coloured till they degenerated into negroes. The contrary would happen to negroes transported to cold climates.

Dr. Wells conceives that the woolly hair, and deformed features, of the negroes, are connected with want of intellect. The negroes have been always slaves; and there is no instance of their better shaped neighbours being subject to the negroes.

The same evening a paper by I. Berzelius and Dr. Marcet, on the *alcohol of sulphur*, was begun. This substance was discovered several years ago by Lampadius, while distilling a mixture of charcoal and pyrites. He called it alcohol of sulphur from its great volatility. He conceived it to be a compound of sulphur and hydrogen. Clement and Desormes obtained it soon after by passing sulphur through red-hot charcoal; and from their analysis it appeared to be a compound of sulphur and charcoal.

Berthollet, in his elaborate remarks upon carbonic oxide, and his critique upon the experiments of Clement and Desormes, revived the opinion of Lampadius; and this was confirmed by the experiments of Berthollet, jun. The subject was lately resumed by Clusel, who concluded from his experiments, that the substance was a triple compound of sulphur, charcoal, hydrogen, and azote. Thenard examined the substance anew, and found it a compound of sulphur and charcoal alone. These discordant results prevent any confidence from being put in the various experiments already made. It was to remove the doubts still hanging over the subject that Berzelius and Marcet were induced to undertake the investigation of the subject.

LINNÆAN SOCIETY.

On the 6th of April the remainder of Mr. Keith's paper on the cotyledons of grasses was read. He found by examination that the sheath of the plumula never assumes the form of a true leaf; that it rises out of the ground, is at first white and transparent, and then becomes purple; and that it divides, and allows the leaf of the plumula to pass through it. The secondary shoots are also furnished with sheaths; but their structure is quite different from that of the plumula. As to the scale called *vitellus* by Gærtner, Mr. Keith conceives that it may be intended to act as a strainer to the milky food prepared in the albumen.

At the same meeting an analysis of *arragonite*, by the Rev. John Holme, of St. Peter's College, Cambridge, was read. This mineral has been long known to mineralogists, and constitutes an anomaly in the Haüyan theory of crystallization. The figure of its crystals, its specific gravity, its hardness and lustre, differ from the same properties in calcareous spar; yet its constituents, as far as chemical analysis has gone, are absolutely the same. It has been analysed by Klaproth, Bucholz, Vauquelin, Chenevix, Thenard, and Biot; but nothing different from the constituents of calcareous spar was found. Mr. Holme conceives the difference to depend upon a quantity of water chemically combined in arragonite, while calcareous spar is destitute of that constituent. He found that when exposed to heat it gives out water without decrepitating, and at the same time falls to powder. This water was found to contain no carbonic acid gas, nor was any given out under a red heat. The constituents of arragonite, according to Mr. Holme, are as follows:—

Lime	55·5
Carbonic acid	43·7
Water	0·8
	100

It is by no means unlikely that the proportion of water here assigned is too small; for nobody can believe that all the water present can be driven off at a heat below redness. Theory gives us the constituents of arragonite as follows:—

Lime	55·50
Carbonic acid	42·23
Water	2·27
	100

Now it is certainly a very odd circumstance that so small a portion of water should act in this case as the cement. The weight of an atom of anhydrous carbonate of lime is 47·9, and that of an atom of water 8·5. Hence it would appear that every atom of water in the arragonite is surrounded by eight atoms of carbonate. This might be conceived to form a cube; but never could constitute the tetrahedron which Haüy conceives to be the form of the integrant molecule of arragonite. Mr. Hulme proposes to call arragonite *hydrous carbonate of lime*.

On the 20th of April a fossil chama car, filled with primitive crystals of carbonate of lime, supposed to be from Wiltshire, was exhibited by Mr. Sowerby.

A fossil turtle, from a quarry in Dorsetshire, was exhibited by Mr. Bullock. The specimen was very perfect, and exhibited the shell of the turtle almost complete. The quarry, from the pieces of stone attached to the specimen, I conceive to be limestone. Only another specimen of fossil turtle was found in this quarry, and it was broken in taking it out.

A letter from Mr. Heyne was read, giving an account of a very singular change which takes place daily in the leaves of a species of cotyledon from India, which is cultivated in our hot-houses. In the morning these leaves are as sour as the leaves of sorrel, at noon they are tasteless, and in the evening they are somewhat bitter. Mr. Heyne explains this singular change by supposing that the plant absorbs oxygen gas during the night, and forms an acid which is again decomposed during the day. Though this explanation be very unlikely to be the true one, the phenomenon certainly deserves to be particularly examined. What is the acid? and what becomes of it?

A paper by Mr. Annesley was read, giving a description of a new species of rubus, which he observed first in Wales, and afterwards in Perthshire and Aberdeenshire. It is the same which was observed by Mr. Hall at Loch Ness, and which he described in the Transactions of the Royal Society of Edinburgh. It is found likewise in Yorkshire, and many other parts of Great Britain.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the French Institute for 1812.

(*Continued from* N° IV. p. 316.)

Physical Department. By M. Le Chev. Cuvier, Perpetual Secretary.

Physics and Chemistry.

Every one knows that heat is one of the principal instruments of chemistry, and one of the greatest forces which act in its phenomena. We may consider it in itself, in its effects, and in its sources.

Count Rumford, who is continually occupied with the sciences, as far as they contribute to the good of society, has this year treated of heat under this last point of view, and has endeavoured with much care to determine how much heat is produced by the combustion of each substance.

To attain this object, it was necessary in the first place to have a general method of measuring exactly these quantities of heat; and when we reflect on the complicated nature of the phenomena of combustion, we must be sensible of the numerous difficulties which Count Rumford had to encounter in his attempts. It was only after a laborious investigation of 20 years that he was able to overcome them.

His principal idea was to measure the quantity of water which passes from one fixed degree of temperature to another equally fixed by the combustion of a measured quantity of each substance. The apparatus which he has contrived for this purpose consists in a prismatic and horizontal receiver of copper, in which there are two holes: one near one of the ends, to receive a thermometer; the other in the middle of the upper surface, through which water is poured in, and which is stopped by a cork. Within this receiver there is a kind of flat worm, which covers the whole bottom without touching it, and which is destined to receive the aerial products of combustion by means of a vertical funnel soldered to its orifice. This worm returns three times on itself, and its other extremity traverses horizontally the upper surface of the receiver, to which it is contiguous. The goodness of the whole apparatus depends upon the flat form of the worm, which ought to transmit to the liquid contained in the receiver all the portion of heat which it receives from the substance that is burnt.

But the receiver, when once hotter than the surrounding air, must lose a portion of the heat which it has received; and the azote of the air which has served for the combustion, being with the other products in the worm, must likewise retain a portion of that heat. To remedy these two causes of error, Count Rumford conceived the simple and efficacious idea of beginning his experiments at a determinate degree below the temperature of the ambiant air, and to stop them when the water of the receiver has reached an equal number of degrees above that temperature; so that in the first part of the experiment the surrounding air and the azote furnish just as much heat to the receiver as they take from it in the last part of the experiment.

The cylindrical reservoir of the thermometer has precisely the same height as the receiver; so that it indicates precisely the mean temperature of the whole water in the receiver.

Count Rumford, by means of this apparatus, burnt successively different combustibles, taking care that the combustion was complete, that no residuum was left, and that neither smoke nor smell was emitted during the combustion. He found that a pound troy of each combustible, during its combustion, raised the heat of the following quantities of water from the freezing to the boiling point:—

White wax	72108 lbs. troy.
Olive oil	68900
Oil of colza	70906
Alcohol	51400
Sulphuric ether	61178
Naphtha	55900
Tallow	63755

It is remarkable that if we admit the accuracy of the analyses of these substances made by Lavoisier, Cruikshank, De Saussure, Gay-Lussac, and Thenard; and if we calculate the heat that would have been produced by the hydrogen and carbon which enter into their composition, if they had been burnt separately, we obtain very nearly the same results.

We cannot make the reader sensible of all the merit of these researches, unless we were to state the numerous calculations of the author, which is incompatible with the nature of our general view.

Furnished with this previous knowledge, Count Rumford passed to the quantity of heat evolved by the combustion of wood; but here the problem became more complicated. A high temperature produces numerous changes in wood. One part of its constituents is driven off, while another enters into new combinations. It was necessary, therefore, in the first place, to examine the structure of wood, the specific gravity of its solid parts, the quantity of liquids and elastic fluids which it contains in their different states, and finally what charcoal furnishes.

After having exactly dried different specimens of wood in a stove, Count Rumford obtained this singular conclusion, that the specific gravity of the solid matter which constitutes the *timber* of wood is almost the same in all trees. By the same means he determined that the woody part of oak in full vegetation is only four-tenths of the whole. Air constitutes one-fourth of it, and the rest consists in sap. Light woods have still a much less quantity of solid matter; but the season of the year, and the age of the tree, occasion considerable variations. Ordinary dry wood contains above one-fourth of its weight of water. Even the oldest wood, though in the state of timber for ages, never contains less than one-sixth of its weight of water.

Count Rumford has determined, by exact experiments, that all absolutely dry woods give from 42 to 43 per cent. of charcoal. Hence he concludes that the ligneous matter is identic in all woods. This loss, which the driest wood experiences when charred, the absolute quantity of carbon determined by Thenard and Gay-Lussac at 52 or 53 per cent., the matters which are deposited on the vessels, and finally this fact that wood too much dried, too nearly approaching to the state of charcoal, gives out less heat—all these circumstances induce Count Rumford to believe that the proper charry fibre, which he calls the woody skeleton, is surrounded by another substance, which he compares to the muscles, and which he calls *vegetable flesh*. The fire first attacks this envelope, because it contains hydrogen, which renders it more inflammable, and which contributes a great deal to the heat given out by wood.

From numerous experiments and complicated calculations, Count Rumford has drawn up a table of the quantity of water which the different woods, according to their state of dryness, can heat from the freezing to the boiling temperature. From this table it appears that the lime-tree gives out the most heat; and the oak the least, during combustion. From the same analyses it follows that the inevitable loss of heat during the charring of wood is more than 42 per cent., and by the ordinary processes of the charcoal-makers more than 64 per cent., because they form a considerable quantity of pyrolignous acid, which consumes this great proportion of carbon. It follows, likewise, that all the charcoal furnished by any wood whatever, furnishes only one-third of the heat that is furnished by the wood itself from which it was formed.

Count Rumford conceives, likewise, that he has ascertained this important fact for chemistry, that carbon may combine with oxygen, and form with it carbonic acid, at a much lower temperature than that in which it burns visibly.

The difficulty of following this philosopher in his complicated calculations respecting the greatest intensity of heat which it is possible to produce, and on the quantity of heat evolved by the condensation of the vapour of water and alcohol, obliges us to notice only the general results. He determines, for example, that the temperature of water at the moment of its formation by the combination of oxygen and hydrogen is eight times higher than that of iron heated so as to appear red in broad day-light; and that boiling water, in passing to the state of vapour, renders latent 1040 degrees of heat, or, which comes to the same thing, that this quantity is evolved when the vapour of water is condensed.

According to the same experiments, the capacity of the vapour of water for heat diminishes with its temperature; and from the phenomena relative to the vapour of alcohol, we may conclude that the oxygen and hydrogen which enters into the composition of this liquid are not in the state of water.

The Class had proposed, as one of its physical prizes, the determination of the capacity of oxygen gas, carbonic acid gas, and hydrogen gas, for heat. This prize has been voted to a memoir of M. M. François Delaroche and Berard. These two philosophers have not satisfied themselves with the cases proposed; they have taken a general view of the matter, and determined the specific heat of other gases; and that of air and vapour under different pressures. Among other interesting particulars, they have found that the capacity of a given mass of air increases with its bulk. Reducing all the capacities to that of water, they have drawn up the following table of their labours:—

Capacity of	Water	1·0000
	Atmospheric air	0·2669
	Hydrogen gas	3·2936
	Carbonic acid gas	0·2210
	Oxygen gas	0·2361
	Azotic gas	0·2754
	Nitrous oxide gas	0·2369
	Olefiant gas	0·4207
	Carbonic oxide gas	0·2884
	Aqueous vapour	0·8470

Heat penetrates all bodies. It contributes essentially to their dilatation, and it is squeezed out, to use the expression, whenever they are reduced, by any operation whatever, to smaller dimensions. Thus we know, by experiments made ten years ago at Lyons by M. Mollet, that air suddenly compressed gives out heat, and that this heat is accompanied with light. This phenomenon has given origin to the convenient instrument by which tinder is kindled by the pressure of a piston.

M. Dessaignes, an ingenious philosopher of Vendome, in a memoir of which we have given an account, having subjected different gases to the same operation, obtained similar effects. Hence it was concluded, apparently with reason, that the same effects ought to appear with all the aeriform fluids. But M. de Saissy, a physician in Lyons, having repeated the experiments of M. Dessaignes, could only produce light with oxygen gas, muriatic acid gas, and common air. Oxygen gas gives the most light, muriatic acid gas comes next in order, and common air gives the least of the three. The other gases do not become luminous, except when some oxygen is mixed with them. M. de Saissy concludes from this that the aeriform fluids have not the property of giving out light by compression, except when they contain oxygen free, or feebly combined. He thinks that this fact, when once well established, will give additional probability to the opinion that heat and light are different substances.

The doctrine of M. le Comte Berthollet on the different actions which influence the definite results of chemical phenomena, depends in a great measure upon this almost general fact, that an alkali which decomposes a saline compound only deprives it of that portion of its acid to which it owed its solubility, and as soon as it becomes insoluble it precipitates, preserving the rest of its acid, and frequently taking with it even a portion of the alkali which acted on it; so that the precipitate is almost always a compound. Yet M. Taboalda had announced that the pure alkalies throw down from *corrosive sublimate* an oxide of mercury free from all acid. M. Berthollet repeated this experiment, and found that the precipitate is not pure unless more alkali be added to the solution of corrosive sublimate than is necessary to saturate all the muriatic acid present. When this is not added the precipitate always retains a portion of acid, which varies according to circumstances. The kind of alkali is indifferent. But when the potash, for example, is completely saturated with carbonic acid, it does not decompose the corrosive sublimate. If we employ a subcarbonate, it acts till it has lost its redundant potash; but the precipitate contains both muriatic acid and potash.

The alkalies produce the same effects on the nitrate of the peroxide of mercury, and experiments made on the sulphate of alumina gave analogous results; that is to say, they confirm the law established by M. Berthollet.

The same philosopher had made experiments long ago to determine the proportion of oxygen and muriatic acid which constitute oxymuriatic acid; but Mr. Chenevix having obtained different results, M. Berthollet returned again to the same subject. He found that the light which he had at first employed as the principal agent takes only a certain quantity of oxygen from the acid, though it reduces it to a state in which its action on reactives differs little from that of common muriatic acid. Hence he concludes that this state is the first degree of oxidation of the muriatic base. Decomposing the oxymuriatic acid completely, by means of ammonia, he found 23·64 per cent. of oxygen instead of 9·41, which was the result of his first analysis.

In one of his preceding memoirs M. Berthollet had stated facts from which it was easy to conclude that carbureted hydrogen gases existed; but he had neglected to draw that conclusion. The analysis of olefiant gas by M. de Saussure, has set that truth in a clear light, by showing that this gas contains no oxygen, and that it is a real carbureted hydrogen gas, composed of 86 parts of carbon and 14 of hydrogen.

Mr. Dalton, in treating of this subject in his New System of Chemistry, has endeavoured to prove that hydrogen and carbon combine only in two proportions. The one gives us olefiant gas, the other the gas of marshes. He considers the gases named by Berrbollet *oxycarbureted hydrogen*, as mixtures of carbureted hydrogen, carbonic oxide, and hydrogen. According to Dalton, olefiant gas, when exposed to heat, or to the action of electricity, passes to the state of the gas of marshes, by depositing one half of its carbon; and the gas of marshes, when exposed to the same action, is entirely decomposed. If we obtain a peculiar gas before that decomposition is complete, this gas is a mixture of hydrogen and the gas of marshes.

M. Berthollet has repeated these experiments with electricity; but they have not led him to the results announced by Dalton. A part only of the gas was decomposed, and that which remained undecomposed resisted the most violent action of electricity. M. Berthollet concludes, likewise, contrary to the opinion of Dalton, that the small quantity of azote which is found in the gas of marshes is a constituent part of that gas; for this gas, collected at very different periods, always contained the same proportion of azote.

M. Berthollet, having exposed olefiant gas to the action of heat, did not obtain the results announced by Dalton. Far from finding only two compounds between hydrogen and carbon, he found, on the contrary, that they are capable of uniting in indefinite proportions, which vary according to the degree of heat which they have experienced.

M. Berthollet likewise exposed oxycarbureted hydrogen gas to the action of heat, and obtained results analagous to those just mentioned. It deposited carbon, and its specific gravity diminished. Carbonic oxide gas was exposed in a red-hot tube to the action of hydrogen, without undergoing decomposition. This is inconsistent with the opinion of Dalton, who considers oxycarbureted hydrogen as a mixture of carbonic oxide gas and carbureted hydrogen gas; for in order to explain this experiment by the hypothesis of Dalton we must ascribe all the changes which heat produces upon oxycarbureted gas to the carbureted hydrogen which it contains, which is very difficult, M. Berthollet having proved by a direct experiment that hydrogen has no action on carbon.

M. Thenard has made very singular experiments on ammoniacal gas, nearly inexplicable in the present state of chemistry. If we expose this gas in a state of purity to heat in a close porcelain tube, very little of it undergoes decomposition; but the decomposition goes on very rapidly if we put into the tube iron, copper, silver, gold, or platinum. These metals undergo a change in their physical qualities, but neither increase nor diminish in weight, neither take from nor give out to the gas any thing ponderable. Iron possesses this property in the highest degree. All the other metals (except the five above-mentioned) are destitute of the property altogether. The gas decomposed by this singular method consists of three measures of hydrogen to one of azote. Sulphur and charcoal likewise decompose ammonia; but form with its elements new combinations.

A metal cannot be dissolved in an acid without being oxydated. Sometimes it takes the oxygen from the acid itself, sometimes from water. It sometimes happens that a solution saturated with a metal in an acid, when assisted by heat, is capable of dissolving a new portion of the metal? Proust discovered this to be the case with the nitrate of lead. In this case is it the acid or the oxide which furnishes oxygen to this new portion of metal? M. Proust and Dr. Thomson, who repeated his experiments, thought that the oxygen came from the oxide; from which it would result that the whole of the lead thus dissolved would contain a smaller portion of oxygen, or, in other terms, that it would be less oxydized than the oxide which enters into the common nitrate of lead, which is the yellow oxide.*

But M. Chevreul, assistant naturalist to the Museum of Natural History, having again examined this question, found that nitrous gas is disengaged when new lead is dissolved in this way; which could not happen unless the nitric acid lost oxygen;

* Cuvier here states the very opposite of the opinion which I gave in my paper on the *Oxides of Lead.* My object was to show that there is no oxide of lead containing less oxygen than the yellow. In the next edition of my System of Chemistry I expressed myself with hesitation on the subject, because Gehlen, in his German translation of my paper, had affirmed, that by repeating my experiments on a greater scale he had detected an oxide containing less oxygen than the yellow. T. T.

from which this chemist concludes that it is the acid which furnishes oxygen to the new portion of lead, and that the solution is changed from the state of a *nitrate* to that of a *nitrite*. A remarkable property, which serves to distinguish the nitrites of lead from the nitrates, is that of forming in the nitrate of copper a precipitate composed of the hydrate of copper and of lead. By these experiments M. Chevreul restores to the yellow oxide of lead the rank of *protoxide*.

This chemist has been led to examine in a general manner the salts which lead forms with nitric acid. He has shown that there are two nitrates and two nitrites; one of which in each class contains twice as much oxygen as the other. He suspects that there exists a third species of nitrite containing four times less oxide than the first.

Porous bodies absorb gases in different proportions, and charcoal is one of those that absorb the most. The accurate knowledge of the limits of this absorption being important in chemical operations, M. de Saussure has lately examined it with much care and success. All charcoals have not that property in the same degree, and all gases are not absorbed in the same proportion. The same charcoal will absorb 90 times its bulk of ammoniacal gas, and scarcely 1·75 of hydrogen gas.

M. Thenard has repeated these experiments with some variations, and has obtained nearly the same results. He has thrown the whole into the form of a table. He has observed, as Saussure and Count Rumford had done in other experiments, that oxygen gas is changed into carbonic acid gas, though the temperature be not high. Nitrous gas is partly decomposed, and carbonic acid and azotic gas disengaged. But sulphureted hydrogen is the gas the absorption of which presents the most remarkable phenomena. It is destroyed in a short time, water and sulphur deposited, and so much heat evolved that the temperature of the charcoal is greatly elevated.

M. Lampadius, a German chemist and philosopher, while distilling iron pyrites with charcoal, had obtained a substance, liquid and volatile, the composition of which was doubtful. Lampadius himself, and the late M. Amedée Berthollet, considered it as a compound of sulphur and hydrogen; M. M. Clement and Desormes, as a compound of sulphur and charcoal. M. Clusel, operator of chemistry in the Polytechnic School, wishing to ascertain the nature of this substance, attempted to decompose it by making it pass over plates of copper in hot tubes; but this method not having entirely succeeded, he endeavoured to analyse it by means of the Voltaic battery, and after many attempts, delicate and numerous precautions, and a skilful use of different chemical bodies, he conceives that he has determined its composition as follows:—

Sulphur	59
Charcoal	29
Hydrogen	6
Azote	7
	100

But he found in his products more sulphur and charcoal than he had employed in his experiment.

M. Thenard resumed the first method of Cluzel, which, being much less complicated, promised more decisive results. By making the liquid of Lampadius pass more slowly over copper in hot tubes, he completely decomposed it into 85 or 86 sulphur, and 14 or 15 charcoal, without either azote or hydrogen.

It will be seen in our preceding reports that M. Delaroche was employed in ascertaining by new experiments the phenomena which animals present when exposed to a high temperature. He ascertained that the cutaneous and pulmonary evaporation was one of the causes which prevented animals from assuming completely the temperature of the surrounding medium; but that they did not preserve their own temperature unaltered, as had been said, but became hotter by degrees. But it was observed that if the temperature of animals increased as that of the surrounding medium, they ought to reach a still higher temperature, because to that of the medium they ought to join that which is produced by respiration.

M. Delaroche, therefore, wished to determine the difference which the result of respiration, or in other terms, the absorption of oxygen, would undergo in an air more or less heated, and he found it so small that it is difficult to draw any conclusion. It is in the proportion of five to six. M. Delaroche conceived that there might be no connection between the frequency of respiration and the chemical phenomena of that process; for in a hot air the number of respirations was greatly increased. An interesting remark is, that cold blooded animals show a much greater difference than others, and that heat sensibly increases the activity of their respiration—a fact which may assist us to explain several phenomena of their economy.

The calculi which occasionally form in the gall bladder, and which have hitherto resisted all the skill of the physician, are usually composed of the substance called *adipocire* by chemists, because its characters resemble both those of tallow and of wax. But it appears that they are likewise subject to vary in their nature; for M. Orsila, a doctor of medicine, has analysed some quite different, which contained no adipocire, but were composed of yellow matter, green resin, and a small quantity of picromel.

M. Vauquelin, continuing his researches on vegetable principles, has subjected the *daphne alpina* to numerous experiments. This shrub is known by the excessive acridity of its bark, which is employed in medicine as a rubefacient, and the extract of which mixed with fatty matter forms a pomatum, which in many cases is substituted for that of cantharides. By digesting this bark in alcohol and water he discovered in it two new principles of a very remarkable nature.

The first, which Vauquelin calls the *acrid principle*, is of an oily and resinous nature. Not becoming volatile but at a heat superior to that of boiling alcohol, it does not rise with that liquid, but may be distilled over with water.

The second principle, named *bitter principle*, is soluble in boiling water; and on cooling, shoots into white crystals having the form of needles.

The bark of the *daphne* yielded besides, like that of many other plants, a green resin, a yellow colouring matter, a brown substance containing azote since it yielded ammonia, and salts with a base of potash, of iron, and of lime.

M. Vauquelin terminates his memoir with this important observation, that the acrid and caustic vegetable substances are oily or resinous, and contain no acid, in which respect they agree with poisonous plants. Hence he concludes that we ought to suspect those plants as not fit for eating which contain no acid.

Reaumur had announced more than a century ago that certain fossile teeth acquired a bluish colour, similar to that of the turquoise, when they are cautiously exposed to a graduated heat. M. Sage having observed that prussic acid is obtained by heating a mixture of potash and of the gelatinous substance of the teeth, and that the magnet attracts iron from the powder of calcined teeth, thinks that the blue colour of the western turquoise is due to a real Prussian blue.

MINERALOGY AND GEOLOGY.

The fossil spoils of organised bodies still continue to occupy naturalists.

M. Traullé d'Abbeville has presented to the Class the petrified head of a small cetaceous animal, which appears to have belonged to the whale genus, and which was dug out of the harbour at Antwerp. M. le Comte Dejean, Senator, sent a similar head from the same place to the administration of the Museum of Natural History. Many vertebræ of animals of the same class, and numerous shells, have been found in the same place.

M. Traullé likewise presented a portion of the jaw-bone of a rhinoceros, found on the sand-hills of the valley of the Somme, in the neighbourhood of Abbeville.

M. Daudebart de Ferussac, a young soldier, transported successively by the duties of his function to the most opposite parts of Europe, took advantage of his leisure moments to examine the fossils, and as he has paid particular attention to land and fresh water shells, he attached himself from choice to that sort of soil discovered in the neighbourhood of Paris by M. M. Brogniart and Cuvier, which containing only fresh water shells, appeared to these naturalists not to owe its origin to the sea, as is the case with most other secondary formations.

M. de Ferussac has observed similar beds containing the same shells, and composed of the same substances, in the south of France, in several provinces of Spain, in Germany, and as far as the bottom of Silesia; so that there can hardly be a doubt that these formations are general.

M. de Ferussac, to give more precision to his observations, has examined the shells themselves, has determined their species with great accuracy, and has given good observations on the variations which they may experience, and several happy ideas respecting the character which may serve to distinguish the genera.

M. Cuvier has just published, in four volumes in 4to. with numerous plates, a collection of all his memoirs on the fossil bones of quadrupeds. He has described 78 species, 49 of which were certainly unknown to naturalists, and 16 or 18 are still doubtful. The other bones found in these recent beds appear to belong to animals known. In a preliminary dissertation, the author explains the method which he followed, and the results which he obtained. It appears to him, from facts which he has established, that the earth has undergone several great and sudden revolutions, the last of which, not more remote than 5 or 6,000 years, destroyed the country at that time inhabited by the species of animals existing, and offered for a habitation to the feeble remains of these species continents which had been already inhabited by other beings, which a preceding revolution had buried, and which appeared in their actual state at the time of this last revolution.

(To be continued.)

Article XI.

New Patents.

Richard Green, of Lisle-street, Leicester-square, in the county of Middlesex, sadler's ironmonger; for a stirrup with a spring in the eye, and a spring bottom for the safety of persons riding on horseback, and to prevent their being dragged in the stirrup. Dated March 3, 1813.

ALEXIS DELAHANDE, of Great Marlborough-street, for the production or the making of a green colour, and the application thereof to various useful purposes. Communicated to him by a foreigner. Dated March 3, 1813.

JAMES THOMSON, of Primrose-hill, near Clithero, in the county of Lancaster, calico-printer; for a method of producing patterns on cloth previously dyed Turkey red, and made of cotton or linen, or both. Dated March 3, 1813.

JOHN WHITE, of Princes-street, Soho, in the county of Middlesex; for a machine for cooking without coals or wood. Dated March 3, 1813.

SIR THOMAS COCHRANE, commonly called Lord Cochrane, for a method or methods of more completely lighting cities, towns, and villages. Dated March 3, 1813.

WILLIAM MITCHELL, surgeon, late in Ayr, now in Edinburgh; for an important discovery in the manufacture of soap. Dated March 3, 1813.

JOSHUA STOPFORD, of Belford, in the county of Northumberland, clerk; for a mangle, intended to be called, The Complete Family Accommodation Mangle, for mangling linen and other cloths. Dated March 3, 1813.

FREDERICK HANK, of High Holborn, in the county of Middlesex, musical instrument maker; for improvements in musical instruments. Dated March 3, 1813.

BENJAMIN MERRIMAN COOMES, of Fleet-street, in the city of London, ironmonger; for an improved apparatus for dressing or cooking victuals, and possessing other advantages in lessening the consumption of fuel. Dated March 9, 1813.

WILLIAM HEDLEY, of Wylam, in the county of Northumberland, coal viewer; for certain mechanical means of conveying carriages laden with coals, minerals, merchandise, and other things. Dated March 13, 1813.

BENFORD DEACON, of Cross-street, Islington, in the county of Middlesex, Gentleman; for a method of applying air for domestic and manufacturing purposes, and of employing them in improved fireplaces and bricks. Dated March 13, 1813.

ROBINSON KITTOE, of Woolwich, in the county of Kent, Gentleman; for a double coned revolving axle for carriages. Dated March 13, 1813.

SIGISMUND RENTZSCH, of George-street, St. James's-square, in the county of Middlesex, watch-maker; for a hydrostatical or pneumatical chronometer. Dated March 13, 1813.

GEORGE DUNCAN, of Liverpool, in the county palatine of Lancaster, rope-maker; for several improvements in the different stages of rope-making, and in machinery adapted for such improvements. Dated March 13, 1813.

RICHARD EDWARDS, of the parish of Budock, in the county of Cornwall, Doctor of Physic, and William Williams, of the borough of Penryn, in the same county, surgeon; for a process for extracting arsenic from any of the ores, or other substances in which it is contained, in a purer state than it is at present procured in this kingdom. Dated March 15, 1813.

George Dodd, of South Ville, Wandsworth, in the county of Surrey, engineer; for certain improvements in umbrellas, which render the same more portable and convenient. Dated March 16, 1813.

William Robert Wale King, of Union-court, Holborn-hill, in the city of London, tin-plate worker; for certain improvements in the application of heat to the purposes of boiling water and other fluids, and to other useful purposes, and of the apparatus for performing the same. Dated March 22, 1813.

Colonel William Congreve, of Cecil street, Strand, in the county of Middlesex; for modes of constructing the locks and sluices of canals, basins, or docks, and generally for the transporting of floating bodies from one level to another. Dated March 23, 1813.

Thomas Brunton, of Cooper's-row, Crutched Friars, in the city of London, merchant; for improvements in making or manufacturing of ships' anchors and windlasses, and chain cables and moorings. Dated March 26, 1813.

John Hughes, of Poplar, in the county of Middlesex, excavator; for a method or apparatus for raising gravel or earth from the bottom of rivers and pits, and for screening and delivering the same into barges or other receptacles. Dated March 27, 1813.

Article XII.

Scientific Books in hand, or in the Press.

Major W. M. Leake, of the Royal Artillery, is preparing for press, in 4to., Researches in Greece, containing Remarks on the Modern Languages of Greece.

Mr. I. M. Coley, of Bridgenorth, is about to publish a Practical Treatise on the Remittent Fever of Infants, with remarks on several other diseases, particularly Hydrocephalus Internus.

Mr. Longmire, of Troutbeck, near Kendal, is writing an Essay on Geognosy.

Lieut. Lockett, Assistant Secretary in the College of Fort William, is engaged in some translations from the Elementary Books of the East, in Grammar, Rhetoric, and Logic, which three sciences will form a 4to. volume.

Mr. Thomas Forster has in the press, Researches concerning Atmospheric Phenomena, in One Vol. 8vo.

Article XIII.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap.	Rain.	
3d Mo.										
March 25	N W	30·37	30·28	30·325	47	35	41·0	—		☽
26	N W	30·50	30·37	30·435	51	27	39·0	—		
27	S W	30·47	30·43	30·450	55	32	43·5	—		
28	N W	30·43	30·30	30·365	67	49	58·0	—		
29	S W	30·30	30·10	30·200	66	53	59·5	—		
30	Var.	30·10	29·89	29·995	58	47	52·5	—	1	
31	S E	29·89	29·18	29·535	57	42	49·5	·40		
4th Mo.										
April 1	S W	29·25	29·18	29·215	50	35	42·5	—	·27	●
2	W	29·45	29·25	29·350	52	35	43·5	—	9	
3	S W	29·74	29·45	29·595	48	27	37·5	—	3	
4	S W	29·85	29·74	29·795	54	29	41·5	—		
5	S W	29·85	29·81	29·830	51	40	45·5	—	·13	
6	S W	29·90	29·74	29·820	58	45	51·5	—		
7	W	29·93	29·90	29·915	65	43	54·0	·43		☾
8	E	29·97	29·87	29·920	69	37	53·0	—		
9	S E	30·04	29·97	30·005	66	41	53·5	—		
10	E	30·10	30·04	30·070	65	41	53·0	·26		
11	E	30·14	30·10	30·120	64	35	49·5	—		
12	E	30·23	30·14	30·185	69	42	55·5	—		
13	N E	30·34	30·23	30·285	66	35	50·5	—		
14	E	30·20	30·10	30·150	66	42	54·0	·36		
15	N W	30·20	29·96	30·080	68	42	55·0	—		○
16	N W	29·96	29·77	29·865	66	44	55·0	—		
17	N W	30·10	29·77	29·935	68	41	54·5	—	—	
18	N W	30·13	30·10	30·115	56	42	49·0	·53		
19	N W	30·10	30·10	30·100	64	44	54·0	—		
20	N W	30·10	30·05	30·075	64	40	52·0	—		
21	N	30·09	30·05	30·070	57	32	45·5	—		
22	N E	30·10	30·08	30·090	50	32	41·0	—	·17	
23	N E	30·14	30·10	30·120	45	34	39·5	·40		☽
		30·50	29·18	30·005	69	27	49·11	2·38	—	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Third Month. 27. Hoar frost: large spreading *Cirri.* 28. Temperature 60° in the evening. 29. Overcast sky. 30. A veil of *Cirrostratus,* a. m. The *Cumulus* afterwards showed itself, and a slight shower ensued.

Fourth Month. 1. Stormy, with rain. 2. Hoar frost: a sprinkling of opake hail about sun-rise; several showers of this, and some rain, during the day. 3. Hoar frost: *Cumulus* a. m. showers of snow and of opake hail p. m. 5. a. m. *Cirrostratus:* wet and windy. 10. *Cirrus* and *Cumulus* clouds: the winds increase in strength from the N.E. The mornings have been misty of late, and there have been plentiful dews, in consequence of the great difference between the temperature of day and night. 15. Wind boisterous in the evening. 16. Cloudy a. m. 17. Slight showers. 20. From the 7th of this month we have had summerlike days and cold nights: the roads have become very dusty, and the earth considerably dry. 21. Some clouds of a threatening appearance from the N. E. in the evening, attended with depression of temperature. 22. p. m. Hasty showers, mixed with hail; after which, steady small rain till evening. 23. Cloudy: several scanty hail showers, from large *Nimbus* clouds passing over. During the approach of one of these, a slender, tapering, and somewhat twisted, column, appeared in front, detached from the main body, and reaching down to the earth, in the manner of a water-spout. In a few minutes, by spreading on all sides, it became incorporated with the rest of the shower. This is not a very uncommon appearance; but I have seldom seen it so perfectly exhibited.

RESULTS.

Prevailing Wind Westerly, interrupted (after the middle of the period) by an Easterly current.

Barometer: greatest observed height ..30·50 inches;
Least..................29·18 inches;
Mean of the period....30·005 inches.

Thermometer: greatest height ....69°
Least ....27°
Mean of the period ....49·11°

Evaporation, 2·38 inches. Rain, 0·70 inches.

Tottenham, L. HOWARD.
Fourth Month, 25, 1813.

Errata in our last Number.

Page 307, lines 33 and 36, *for* "Haüy" *read* "Cuvier."

ANNALS

OF

PHILOSOPHY.

JUNE, 1813.

Article I.

Biographical Account of the Rev. Nevil Maskelyne, D. D. Astronomer Royal. By M. le Chevalier Delambre, Secretary of the French Institute. *

Nevil Maskelyne, Doctor of Divinity, member of Trinity College, Cambridge, Fellow of the Royal Society, one of the eight Foreign Associates of the Academy of Sciences, and of the Class of Physical and Mathematical Sciences of the Imperial Institute, Astronomer Royal of England, was born in London on the 6th of October, 1732, of an ancient family which had been long established in the west of England.

At the age of nine he was placed at Westminster school, where he speedily distinguished himself. He showed an early taste for optics and astronomy; but what decided his vocation was the eclipse of the sun of 1748, which was of ten digits in London. It is remarkable that this eclipse produced the same effect upon Lalande, who was only three months older than Maskelyne. We may say with truth that never was celestial phenomenon more useful to the science than the eclipse which furnished it with two astronomers so singularly distinguished, though in different ways: one of whom wrote a great deal, was long a professor, and formed a great number of pupils, but observed very little; while the other wrote less, but has left us, in the collection of his observations, the greatest and most valuable monument of the kind which exists.

Maskelyne perceived how necessary the mathematics were in the career which he proposed to run; he set himself accordingly

* Translated from the Moniteurs for the 5th and 6th of March, 1813.

to study them, and acquired in a few months the elements of geometry and algebra. This first success was the earnest of what he could not avoid obtaining by reading the principal treatises on astronomy and the higher analysis, with which he employed himself habitually. At this time he went to Cambridge, and entered first into Catharine's Hall, and afterwards into Trinity College, where he received with eclat the title of Bachelor of Arts.

In 1755 he accepted of a curacy in the neighbourhood of London, where he resided for some years, employing the whole of his leisure time in his favourite study. About this period he connected himself with the great astronomer Bradley, for whom it appears he made different calculations of importance. In 1758 he became Fellow of Trinity College, Cambridge, and the next year he was elected a Fellow of the Royal Society.

But it was in the year 1761 that his real astronomical career began, when he was chosen to go to the island of St. Helena, to observe the transit of Venus over the sun's disk. To render this voyage the more useful he offered to the Royal Society to make observations on the parallax of Sirius. This beautiful star had been often observed by La Caille at the Cape of Good Hope. Dr. Maskelyne, from calculating these observations, thought he saw proofs for the existence of a parallax of 4·5″, from which it would result that Sirius is not nearly so far distant from the earth as was commonly imagined. But while he did full justice to our celebrated astronomer [La Caille], he observed that these observations made with another object in view, were neither sufficiently numerous, nor made in circumstances sufficiently favourable, to determine the point with exactness; and that the variations which we remark in these observations, though pretty regular in general, might however proceed, in part at least, from inevitable errors of observation.

The Abbé de la Caille, being informed of the project of Maskelyne, wrote to Warton, their common friend, to recommend the transit of the moon over the meridian to verify likewise the parallax of that luminary, which he had himself determined at the Cape of Good Hope with so much care and success. He sent him at the same time a note of the observations which he thought useful; thus giving a striking proof of that love for truth to which he sacrificed on all occasions his time, his repose, and even his self-partiality.

Dr. Maskelyne on his side took similar precautions, and without knowing that he was anticipated, sent a note to the French astronomers containing the observations which he recommended them to make, as La Caille had done eight years before.

Clouds prevented the observation of the transit of Venus, which had given occasion to the voyage; but Maskelyne, furnished with an excellent pendulum of Shelton, which had been regulated at Greenwich by Bradley, and which had been transported with the greatest possible care, determined the number of oscillations which it made less in St. Helena than at London, in order to deduce from that observation the diminution of gravity.

The secondary object of the voyage, the parallax of Sirius, likewise failed; but it produced an observation both curious and useful. To know if Sirius had a sensible parallax, it was necessary to have a more perfect instrument than that of La Caille; it was necessary to observe the star in peculiar situations. The first of these requisites depended upon the artist, the second upon the astronomer. The Royal Society had got a sector made on purpose, which was only finished just when the vessel sailed, and could not be verified at Greenwich. What was the surprise of Maskelyne when he found that this instrument, destined for the most delicate researches, gave him from one day to another differences of 10″, 20″, and even 30″, in the measure of the same angle. In examining with care what could be the cause of these singular variations, he discovered it without difficulty, made himself certain of it by various proofs, and endeavoured to correct it, but could succeed only imperfectly. He reduced the error to 3″, which was far from being sufficient for the object that he had in view.* This obliged him to renounce his second project. The result, however, was an amelioration in the construction of these astronomical instruments.

* This fault was occasioned by the plumb-line forming at its upper extremity a buckle by which it was fixed to a cylinder $\frac{1}{20}$th of an inch in diameter, placed at the centre of the sector. It was impossible to direct the telescope to a star without giving the cylinder a movement of rotation equal to the zenith distance of the star. During this movement, from the effect of adherence, the cylinder displaced the line from its primitive position. Hence the arc which had passed under the line was not the true distance of the star from the zenith. Maskelyne filed down the cylinder to $\frac{1}{70}$th of a line, and then the error was reduced to 3″. It was doubtless upon this occasion that the present mode of suspension was thought of, which consists in fixing the line higher up to a point from which it may hang freely opposite to a point marked on the anterior surface and axis of the cylinder. By this method we are sure that the line preserves invariably the same position, and we may depend upon the distances observed.

We may ask if the same fault did not exist in the sector with which Bradley made his admirable discoveries of the aberration and nutation. The answer would be the same. For the sector of Bradley, the workmanship of Graham, was the model upon which that celebrated artist had constructed the sector carried to Lapland. Hence Bradley could not depend upon the absolute distances which he measured. Fortunately the error was nearly constant for each star that he observed. He required only the relative distances, and the sector gave them almost as exactly as if there had been no error. This fault, which existed certainly in the sector of Lapland, did not prevent Le Monnier, when he returned to France, to observe, as Bradley had done, all the variations produced by aberration, and to confirm fully the brilliant discovery of the English astronomer. See *Degré du Meridien entre Paris et Amiens.* (*Paris*, 1740.)

Instructed by this unlucky experiment, he demanded if the sector of La Caille had not the same fault: but instead of a cylinder, La Caille had a very fine needle, which could hardly occasion a greater error than 2″. He demanded, likewise, if the sector which our [the French] academicians had taken to the polar circle in 1736 was not of the same construction; and his conjecture was likewise just. But the dimensions of the cylinder being only half a line, the resulting errors must have been only one-fourth part of those ascribed to the operation after it was performed a second time by M. Svanberg with the repeating circle.

He was obliged to omit observations on the parallax of the moon, as well as of Sirius. However, to go as far as possible into the views of La Caille, he had recourse to the observation of right ascensions. He was doubtless aware that this mode could not come into competition with that of the French astronomer; for he never mentioned the results which he obtained, though he repeated them a second time in his voyage to Barbadoes.

If he had the regret of seeing all his projects prevented without any fault of his own, he took care at least, after the example of La Caille, to make his voyage useful by determining various longitudes. He tried different methods of resolving these problems; and confirmed all the conclusions drawn by La Caille in favour of the distances of the moon from the sun; and as he had in his possession very accurate instruments, he ascertained that the limits of error were very small. He gave new formulas for calculating these observations, and carried his accuracy so far as to calculate separately the effect of refraction and of parallax.

On his return he published his *British Mariner's Guide*, in which he proposed that Great Britain should adopt the plan of a nautical almanac traced by La Caille after his voyage to the Cape of Good Hope. The same year he made a voyage to Barbadoes, in order to examine the goodness of Harrison's time-pieces. The report which he made at his return, though favourable in general to the celebrated artist whose invention he had subjected to the most severe test, was far from convincing Harrison, who attacked him in a pamphlet. Maskelyne wrote a reply to this attack. Naval men and philosophers took part with one side or other, according to their ideas and their habits. M. de Fleurieu, particularly connected with F. Berthoud, and entirely devoted to the cause of the time-pieces, forgot perhaps on this occasion his accustomed moderation. It was a dispute between two useful methods, calculated to assist each other. Maskelyne did not find the time-pieces sufficiently certain, nor sufficiently regular. Harrison affirmed, not without reason, that they were within the limits prescribed by Act of Parliament. He demanded the whole reward, which was afterwards given him, though at first he received only the half. While pleading his cause he attacked the astronomical methods, availing himself of some admissions of La Caille, who, with his incorruptible integrity, while boasting of the method of the lunar distance, admitted that they had sometimes led him into error. Maskelyne proved by his own observations that the errors are much diminished when better instruments are employed than those used by La Caille, such as were then beginning to be constructed in London. It is possible that in this dispute between mechanics and astronomy both sides went a little too far. The time-pieces performed every thing demanded by the Act of Parliament of 1714, and there can be no doubt that, if they had been presented at that time, Harrison would have obtained the whole reward without difficulty. But 50 years afterwards, when the instruments were much more complete, when the lunar observations had received unexpected ameliorations, was it not excusable to demand a little more? The time-pieces, by the facility which they offered, were likely to seduce maritime men, who are usually enemies to long calculations, but their exactness could only be trusted in short voyages. In less ordinary circumstances, and in long navigations, the method of lunar distances had an incontestible advantage. Hence Maskelyne appears to us to have displayed as much justice as discernment in assigning one half of the reward to Harrison for his time-piece, and the other half to the lunar tables which Meyer before his death had sent to the Board of Longitude in London. The English nation yielded at last to motives of generosity, as much as of justice, in giving to Harrison the whole of the reward to which he had a right, according to the literal meaning of the Act of Parliament. Maskelyne, who at that time laboured to get the Nautical Almanac adopted, had reason to fear that the nation, after having so magnificently rewarded one invention, would become more indifferent and more economical with respect to a work still finer, and of more utility. It was his duty to plead the cause of science, and he performed it with honour. Both parties gained their cause. Maskelyne made his country adopt the plan of La Caille, which that astronomer, too early removed for the interests of the science, could not get introduced into France. The English had the glory of realising it first; and this is an obligation which seamen and astronomers of all nations and ages have to Dr. Maskelyne, who, in order to succeed in it, stood in need of all his perseverance, and of the consideration which he so justly enjoyed. There can be no doubt that to this plan is owing a part of the improvement which the theory of the moon successively received, with which he was continually occupied. He was the editor of Meyer's tables, to which he added tables of the horary motion wanting in the copy received from Gottingen. He compared these tables with the observations that he made every day. It was under his direction that Mason published a corrected and enlarged edition of these tables, brought to perfection afterwards by Burg, and quite recently by M. Burckhardt, who have had the advantage of having recourse on the one hand to thousands of observations made by Maskelyne, and on the other to the analytical researches of Laplace, which furnished them with equations that it would have been difficult to discover among so many others, had there been no other resource but that of observations.

It was the post of Astronomer Royal, to which he was appointed in 1765, which put it in his power to render this important service to the science. The observatory is placed in Greenwich Park, about six miles from London. It was in this retreat that Dr. Maskelyne, for 47 years without interruption, observed the heavens, and collected an inestimable treasure, to which, for these 30 years past, every one has had recourse who wished to improve the tables or the theories of astronomy. For it is not sufficient that an astronomer possesses sufficient courage to employ all his days in calculations, after having consecrated his nights to observations; he must have at his disposal a situation and a set of instruments such as private individuals cannot command, and which are only to be found in establishments founded by governments. This well-known truth occasioned the building of the Observatories of Paris and Greenwich almost at the same time. But in these two celebrated establishments an essential article was forgotten. Maskelyne first thought of remedying that defect, and by that means he rendered an important service to science, which constitutes the principal difference between the destiny of these two rival observatories. There was a difference in their regulations, which could not but produce very sensible effects.

At Paris the architect was chiefly consulted, and at a great expense a beautiful monument was constructed, but indifferently suited for observations. The astronomers, all academicians, formed in it a species of republic without magistrates, where each was employed in labours useful indeed, but without any general plan. The Cassinis, the Lahires, the Maraldis, published from time to time their discoveries, or some interesting result, but they alone were acquainted with their own observations, and others adopted on their word the consequences which they had had time or sagacity to deduce themselves.

At Greenwich the building is less sumptuous, but better adapted for astronomical purposes. There was only a single astronomer, with an assistant. The law which had established the observatory imposed upon the astronomer the obligation to observe every day the sun and the moon, and every thing which could interest geography and navigation.

Flamsteed filled that office for 30 years. A part of his observations was published during his life-time, and his heirs gave afterwards a more complete and accurate edition. At his death, in 1720, he was succeeded by the celebrated Halley, who continued upon the same plan, but with better instruments, till the year 1750; but none of his observations have yet seen the light. In founding the place of astronomer, and in imposing upon him the obligations which he had to fulfil, it had been forgotten to enact the publication of his observations at the end of every year. Such an impression requires a degree of care which the astronomer would discharge with pleasure; but it incurs an expense which he would be unable to support, because the sale of such a collection is of necessity very slow and very limited.

Bradley, succeeding Halley, renewed the instruments, brought the methods to perfection, and made himself celebrated by his discoveries, but published nothing. His heirs pretended that his manuscripts belonged to his family; and it was not till 40 years after his death that astronomers were put in possession of that treasure.

In France the same inattention produced similar effects. About the year 1740 Lemonnier wished to publish an *histoire celeste*, in imitation of that of Flamsteed. One volume appeared, containing the observations of Picard and Lahire down to 1685. This collection, appearing 50 years too late, had lost almost all its value. As long as it might have been useful it remained entirely unknown. Lemonnier promised a second part; but the small sale of the first prevented him from keeping his promise. He obtained as a particular favour that his own observations should be printed in the Louvre; but there remained a blank of 60 years which has never been filled up. M. Cassini had announced an *histoire celeste*, which was to contain the labours of his three predecessors; but the example of what had happened to Lemonnier, perhaps, and the misfortunes of the Revolution, which pressed so severely upon him, prevented him from executing his project. La Caille could find no other means of publishing his *Fondemens de l'Astronomie* than that of calculating gratuitously 20 years of ephemerides for a bookseller, who printed in return as many copies of his book as he wanted, to present one to every astronomer of his time. All the observations which he made after that period remain unpublished.

It is said that the Queen of Great Britain, struck with the small salary allowed the Astronomer Royal for so laborious an employment, had offered to get it increased. Bradley * opposed the proposal, alleging that if the place were worth any thing

* It was not Bradley, but Dr. Halley, who had the merit of the refusal. The Queen alluded to was Caroline, Queen of George II. [*Note of the Editor.*]

considerable it would not continue to be given to an astronomer. The disinterested precaution of Bradley claims our admiration; but if when he refused any thing for himself, he had laid hold of the opportunity of demanding a fund for printing the observations, the Queen would doubtless have acceded to the demand, and he would have saved the disputes which during 40 years prevented the appearance of his labours. Bradley allowed a favourable opportunity to escape, Maskelyne produced one. He procured his observations to be annually printed at the expense of the Royal Society. It was by this means that he deserved to be, as he was for 40 years, the chief, and, as it were, the regulator of astronomers. Piazzi alone was able at last to dispute with him this supremacy; but when we reflect upon the difficult circumstances in which that astronomer has been for so long a time, we shall not be surprised that he published but a small part of his numerous observations.

Since the establishment of the Board of Longitude in France, the observatories of Paris and of Greenwich are directed nearly to the same objects; and, furnished with instruments equally good, they produce annually collections of observations equally precise, which would serve mutually to verify one another if there were occasion for that. They serve as a supplement to each other, when the clouds which cover one observatory do not extend likewise to the other. The communications are continual, and the obligations reciprocal. If our tables are founded in a great measure upon the observations of the English, on the other hand the calculations of the English are founded upon our tables. But the latest tables have been verified by as many French as English observations.

Dr. Maskelyné no more quitted his observatory. In 1769 he remained in it to observe the transit of Venus, though only one phasis was visible at Greenwich; but he drew up instructions for the astronomers whom Great Britain sent to different countries. He collected their observations, and deduced from them the parallax of the sun, and its distance from the earth. His result was the same as that to which Dusejour came by comparing the totality of the observations of the two transits of 1761 and 1769.

He always made the most interesting and most difficult observations himself, as those of the moon; and trusted those only to an assistant which were more easy and less essential. He followed with inflexible rigour the methods established by his celebrated predecessor Bradley, whom he even surpassed in the exactness of his daily observations. He brought to perfection the method of Flamsteed, to determine at once the right ascensions of stars and of the sun. He gave a catalogue of stars, not numerous, but determined with particular care, which has served almost solely during these 30 years for the foundation of all astronomical researches. We may say of the four volumes of observations that he has published, that if by any great revolution the sciences were completely lost, and that this collection was preserved, there would be found in it sufficient materials for rebuilding almost the whole edifice of modern astronomy, which cannot be said of any other collection; because to the merit of an exactness which has been seldom attained, and never surpassed, it adds the advantage of a long series of observations. Its precision is so great that it is very improbable that much can be added to it. The observations are excellent for the time in which they were made, and this time is the period in which they approached the nearest to perfection. They will only increase in value as they increase in age, which unfortunately is not true either with respect to the observations of Tycho and Helvetius, or with those of Flamsteed and La Hire, which when made, possessed all the exactness of which any idea could be formed; but which, though not far removed from the present age, never can enter into any comparison with the observations of the great astronomers of the 18th century.

Dr. Maskelyne corresponded with all the astronomers of the world. To be convinced of it we have only to run over the memoirs of philosophers of every nation which he presented to the Royal Society. He himself did not publish quite so often as could have been wished; but it is very difficult for an astronomer charged with observations to be repeated every day, and almost every moment, to undertake great theoretical researches, which he is under the necessity of interrupting almost every instant. The writings which he has left are remarkable for just ideas and an enlightened criticism. Such is a dissertation on the equation of time, where he has pointed out with the requisite delicacy a mistake of La Caille, and another less important mistake of Lalande. If we may be permitted in our turn to find something reprehensible in his formula, we will acknowledge at least that the trifling negligences to be perceived in it have no sensible effect, and that he allowed them to remain because they were not dangerous.

Lalande received very well the lesson which he thus got; but Bernoulli having inserted, seven years after, a translation of Dr. Maskelyne's memoir in his *Collection for Astronomers*, one of Lalande's pupils (d'Agelet) took up the cause of his master in a manner that might have produced a coldness between the parties concerned. The quarrel, however, had no consequence, and the two astronomers corresponded as usual.

Some doubts were attempted to be raised respecting the latitude and longitude of Greenwich. Dr. Maskelyne, to whom the memoir was sent, showed, with his usual logic and moderation, that the doubts were improper; but he did not oppose the methods proposed to obviate them. It was upon this occasion that the English, who had hitherto done nothing respecting the grand geographical operations in which the French had distinguished themselves, signalised themselves in their turn by methods which surpassed every thing that had been hitherto done. It was then likewise that M. M. Cassini and Legendre made the first trial of the circle of Borda.

Bouguer, at the end of his measure of a degree in Peru had endeavoured to determine the attraction of mountains, and the quantity which they draw the plumb-line of the sector from the meridian. He had found a real and indisputable attraction; but one-half less than ought to have resulted, from the size of the mountain. Hence he concluded that it was hollow within, and undermined by a volcano. Doubts might be entertained of a result obtained by means of instruments of middling goodness. Bouguer had himself expressed a wish that the experiment were undertaken in Europe with more care and with better instruments. Dr. Maskelyne undertook this inquiry, with the sector that he had with him at St. Helena, after having corrected the suspension, and altered the division. He made choice of Schehallien, a mountain in Scotland. It will be necessary to consult his memoir, in order to see the care and the pains which this operation cost him which appears so easy. He found 5·8″ for the derangement of the thread by the attraction of the mountain; he concluded from it that the density of the mountain ought to be one-half of the mean density of the earth. It results from this, that the density of the interior of the earth is greater than that of its surface. This had been already proved by the measurement of degrees, and by the pendulum. Finally, he concluded that the density of the earth is four or five times greater than that of water. Cavendish, by experiments of another nature, found afterwards five and a half for the density of the earth. But he himself had some doubts about the extreme precision of his result, and as that of Maskelyne is likewise founded upon suppositions not rigorously exact, we may, till new experiments be made, suppose the density of the earth to be five times that of water. Finally, Dr. Maskelyne admits it as very possible that the unequal density, even at the surface, may have occasioned the differences observed in the measurement of different degrees.

Such are the principal memoirs published by Dr. Maskelyne, but he left a great many others in manuscript, and philosophers will doubtless learn with pleasure that the care of publishing them has been entrusted to Mr. Vince, Professor of Astronomy and Experimental Philosophy at Cambridge, known by a treatise on astronomy, and by the description of the most modern instruments. We will find perhaps some new details on a micrometer composed of a prism which moves all along the axis of the telescope, like those of M. Rochon and P. Boscovich. According to this last philosopher Maskelyne first conceived the idea of such a micrometer. Boscovich affirms that he also conceived it. It is not without example to find the same contrivance fallen upon by different persons almost at the same time without any communication with each other. Hitherto M. Rochon is the only person who has published observations made with this micrometer. The idea of employing in it double refraction belongs to him incontestibly, as Boscovich himself acknowledges. Dr. Maskelyne employed only a common glass. It seems certain that he first thought of moving the prism in the interior of the telescope. It remains for us to know the advantages which he derived from this construction.

Dr. Maskelyne, who knew the value of excellent instruments, as he was continually using them, turned his whole attention to preserve them properly, and to improve them by the additions suggested by his experience and skill in optics. He made the eye-piece moveable, in order to avoid all parallax in bringing the eye opposite to each of the five wires which the luminary crosses in succession. He discovered the inconvenience of straight trap-doors used in all observatories. He enlarged the size of those at Greenwich, after having shown the necessity of placing the telescopes as much as possible in the open air.

Notwithstanding all these cares it has been lately suspected that his quadrant had become less exact in consequence of the friction which it had undergone during its continual employment for more than 50 years. It was very natural that an astronomer who always paid the same degree of attention to his observations, and who did not perceive in his instrument any mark of old age, should not be the first to detect alterations in it very slight in themselves. Other instruments, more modern, and of a different construction, and placed in the hands of attentive astronomers, occasioned the first suspicions. It is true that the small variations which appear to have been observed may be accounted for in such a way as to acquit the quadrant at Greenwich of inaccuracy. M. M. Besset and Oltmanns gave explanations of them not deficient in probability; but the most certain method was to procure new instruments. This was what Dr. Maskelyne did. He employed the celebrated Troughton to make a grand and superb circle, which he had not the pleasure himself of placing in his observatory; but which he put into the hands of his successor. Mr. Pond will make us acquainted with the faults which age had produced in the Greenwich quadrant, and will inform us what corrections must be made in the latter observations at Greenwich to render them as valuable as the more early observations in the same place. Thus instruments grow old sooner than men, and it is very seldom that an astronomer consents to use those which his predecessor employed.

Dr. Maskelyne died on the 9th of February, 1811, at the age of rather more than 78 years.

The works which he has left, besides his four volumes in folio of observations, the memoirs of which we have spoken, and the first 45 volumes of the Nautical Almanac, calculated under his direction, and revised by him, are, his British Mariner's Guide; the Tables necessary for the usage of the Nautical Almanac; Dissertations on Nautical Astronomy and the use of the Octant; and finally, his posthumous works, of the contents of which we are ignorant, but which astronomers will be very anxious to procure.

Thus we have described the philosopher: but the man, the father, the friend, was not less valuable. Every astronomer, every philosopher, found in him a brother. This is the testimony which M. Chabert gave of him on his return from London, in which he had taken refuge during a season of storms, and where he received the most friendly reception from the Astronomer Royal, accompanied with attentions the most delicate and the most generous. Of a character friendly and amiable, he gained the affections of all those who had the good fortune to know him, and his death was honoured with their regret. Destined at first to the ecclesiastical profession, he preserved always the virtues and the sentiments of that profession. He died as he had always lived, a Christian, firm in his faith, and in the hope that he would be admitted into the presence of a Creator whose works he had so long contemplated and admired.

He left behind him an only daughter, Margaret Maskelyne, to whom we are indebted for materials for this account, of which we could have wished to have been able to make a better use. We hope at least that she will not see without some satisfaction the sentiments of esteem and gratitude with which her respectable father had inspired his brethren in France, and we venture to say in all nations.

Note by the Editor.—The preceding account has been given in the words of Delambre; because it does equal honour to the candour of the French philosopher and to the eminence of Maskelyne. But as the list which it contains of Dr. Maskelyne's papers, published at different times, is far from complete, we think that the reader will see with pleasure an exact list of them. We therefore subjoin it here:—

1. A Proposal for discovering the Annual Parallax of Sirius. Phil. Trans. 1760, vol. ii. p. 889.

2. A Theorem on the Aberration of the Rays of Light refracted through a

Lens, on account of the Imperfection of the Spherical Figure. Phil. Trans. 1761, vol. lii. p. 17.

3. Observations of Jupiter's Satellites, recommended to be made by the French Astronomers. Ibid. p. 26.

4. Account of the Observations made on the Transit of Venus June 6, 1761, in the Island of St. Helena. Ibid. p. 196.

5. Observations on a Clock of Mr. John Shelton made at St. Helena. Phil. Trans. 1762, vol. lii. p. 434.

6. Result of Observations of the Distance of the Moon from the Sun and fixed Stars, made in a Voyage from England to the Island of St. Helena, in order to determine the Longitude of the Ship from time to time; with the whole Process of Computation used on this occasion. Ibid. p. 558.

7. Observations on the Tides in the Island of St. Helena. Ibid. p. 586.

8. Concise Rules for computing the Effects of Refraction and Parallax in varying the apparent Distance of the Moon from the Sun or a Star; also an Easy Rule of Approximation for computing the Distance of the Moon from a Star, the Longitudes and Latitudes of both being given. Phil. Trans. 1764, vol. liv. p. 263.

9. On the Equation of Time, and the true Manner of computing it. Ibid. p. 336.

10. Astronomical Observations made at St. Helena. Ibid. p. 348.

11. Astronomical Observations made at the Island of Barbadoes; at Willoughby Fort; and at the Observatory on Constitution Hill, both adjoining to Bridge Town. Ibid. p. 389.

12. Introduction to Observations made by Messrs. Mason and Dixon for determining the Length of a Degree of Latitude in the Provinces of Maryland and Pensylvania. Phil. Trans. 1768, vol. lviii. p. 270.

13. The Length of a Degree of Latitude in the Provinces of Maryland and Pennsylvania, deduced from the Observations of Mason and Dixon. Ibid.

14. Proportion of English to French Measures. *In a note at the end of the preceding paper.*

15. Observations of the Transit of Venus over the Sun, and the Eclipse of the Sun on June 3, 1769, made at the Royal Observatory. Ibid. p. 355.

16. Description of a Method of measuring Differences of Right Ascension and Declination with Dollond's Micrometer; with other new Applications of the same. Phil. Trans. 1771, vol. lxi. p. 536.

17. Remarks on Hadley's Quadrant, tending principally to remove the Difficulties which have hitherto attended the Use of the Back Observation, and to obviate the Errors that might arise from the want of Parallelism in the two Surfaces of the Index Glass. Phil. Trans. 1772, vol. lxii. p. 99.

18. Deluc's Rule for measuring Heights by the Barometer reduced to the English Measure of Length, and adopted to Fahrenheit's Thermometer, and other Scales of Heat, and reduced to a more convenient Expression. Phil. Trans. 1774, vol. lxiv. p. 158.

19. Observations of the Eclipses of Jupiter's first Satellites, made at the Royal Observatory at Greenwich, compared with Observations of the same made by Samuel Holland, Esq. and others of his party in several parts of North America; and the Longitudes of the Places thence deduced. Ibid. p. 184.

20. A Proposal for measuring the Attraction of some Hills in this Kingdom by Astronomical Observations. Phil. Trans. 1775, vol. lxv. p. 495.

21. An Account of Observations made on the Mountain Schehallien for finding its Attraction. Ibid. p. 500.

22. Of a new Instrument for measuring small Angles, called the Prismatic Micrometer. Phil. Trans. 1777, vol. lxvii. p. 799.

23. The Longitude of Cork settled. Phil Trans. 1779, vol. lxix. p. 179.

24. Advertisement of the expected Return of the Comet of 1532 and 1661 in the Year 1788. Phil. Trans. 1786, vol. lxxvi. p. 426.

25. Concerning the Latitude and Longitude of the Royal Observatory at Greenwich; with Remarks on a Memorial of the late M. Cassini de Thury. Phil. Trans. 1787, vol. lxxvii. p. 151.

26. An Attempt to explain a Difficulty in the Theory of Vision, depending on the different Refrangibility of Light. Phil. Trans. 1789, vol. lxxix. p. 256.

27. Observations of the Comet of 1793. Phil. Trans. 1793, vol. lxxxiii. p. 55.

28. An Account of an Appearance of Light, like a Star, seen lately in the dark Part of the Moon by Thomas Stretton in St. John's-square, Clerkenwell; with Rremarks on this Observation and Mr. Wilkins's. Phil. Trans. 1794, vol. lxxxiv. p. 435.

Article II.

Remarks on the Transition Rocks of Werner. By Thomas Allan, Esq. F.R.S. E. Read to the Royal Society of Edinburgh in 1812.

Although we have many writers on geological subjects, whose works are distinguished by ingenuity of doctrine, and novelty of opinion, and, among them, some who have made advances towards arrangement, it was reserved to the celebrated Werner, to introduce means, by which rocks might be described with some degree of precision. Many ingenious theories were invented, to account for their formation; but little or no attention was paid to the acquirement of an accurate knowledge, either of their composition, or their relative position in nature; although these certainly appear to be the bases, on which such speculative opinions ought to be founded.

But while we acknowledge these obligations to the Professor of Freyberg, we cannot extend our unqualified approbation to the *systematic arrangement* he has introduced. It was not to be expected, that the labours of one individual, who, from peculiar circumstances, was confined within certain limits,* were sufficient to attain perfection; nor could it reasonably be supposed, that any district, however extensive, should be so singularly favoured, as to contain all the variety of facts, that occur in other parts of the world, from which deductions are to be drawn, and elucidations afforded, investing phenomena with characters which they do not present elsewhere.

In forming his arrangement, Werner may have exhausted the means he possessed; he therefore ought not to be reproached, for although his conclusions are more general than are warranted by the circumscribed field to which he was confined, yet he has formed a groundwork on which the labours of future geologists may rear a system more capable of affording satisfaction.

* In Werner's preface to his Theory of Veins, he states, that his limited fortune, and the nature of his present situation, prevented him from travelling into more distant countries. Anderson's Translation, xxiii.

It is greatly to be wished that arrangements of this kind were less dictated by theory. The pupils of the Wernerian school have been peculiarly fettered by an ideal necessity of supporting the principles of their master; but the blending of theory with description is an error common to all speculative geologists, the support of preconceived opinions being very generally the principal object in view. Hence we find that collections of those facts which are supposed favourable to certain doctrines have been eagerly pursued, and others, equally interesting in themselves, entirely overlooked; while that minute detail, which is alone capable of placing the student in a situation to draw conclusions of his own, has been totally neglected.

The part of the Wernerian system which it is my intention to notice at present, is the class of rocks termed *transition*. After stating the grounds on which this distinction has been established, and the particular rocks of which the series is composed, with their extent and importance, I shall endeavour to show that those which constitute its principal members are similar in different districts; and, finally, that they are of an older date than granite, which maintains the first place in point of priority in the system of Werner.

It is well known that one of the principal arguments brought forward by Dr. Hutton is drawn from the penetration of the stratified rocks by veins extending from the mass of granite, which he considered as affording a decisive proof of the subsequent formation of that rock. It must not therefore be supposed that I aim at any thing original in the above assertion, or that I even wish to limit the term *alpine schistus*, as applied by that ingenious philosopher; there can be no doubt that under this name he included both the primitive and transition stratified rocks of Werner; but in his time no distinction had been drawn between them: it is only later discoveries that have imposed the necessity of more specific language, which may at once account for that want of precision by which his writings are so much obscured, and the deficiency of mineralogical knowledge with which he has been so frequently charged.

Werner, in the construction of his systematic arrangement, thought that he perceived grounds for considering all rocks, from granite down to clay-slate, as bearing marks of having been deposited from the original chaotic fluid in a certain determinate order. In them no detritus, or any thing like organised nature, was to be observed; and to this point every rock remained exactly in the same state, in which it was at the period when it first acquired solidity. To these alone the title of primitive was attached. In the rocks immediately following, of which limestone is said to be the first, he remarked an essential difference; the limestone not only abounded in organic remains, but other members of the series were composed of fragments, which must have existed previously in a different state: hence he inferred that these rocks were formed at a subsequent period; which, from their constituent parts, he concluded must have been after the creation of living animals, and nearly at the time when the earth passed from its chaotic to its habitable state;* and on these grounds he distinguished this class by the name of transition.

To this another class succeeded, also presenting new and distinct characters, one of the most remarkable of which is position. They are never found conformable with the transition rocks; while these present an uneven or serrated outline, either from the natural contortions of the strata, or the broken edges of the highly inclined beds; the rocks which succeed fill up the inequalities, and assume an horizontal position. To them he gave the name of floetz rocks.

Thus the system is divided into three great classes, the primitive, transition, and floetz.

Although the transition has been known in this country as a separate class only within a few years, yet it occupies a larger superficial extent in these islands than any other rock formation. But before I proceed to trace its limits, it may be proper to explain what is understood by the transition series. In doing this, and indeed in whatever else I have stated with respect to the Wernerian geognosy, I beg to be understood as having taken it from that work, which I consider as containing the most authentic account of the system taught at Freyberg; I mean the third volume of Professor Jameson's Mineralogy. As Werner has published no account of it himself, it is only from the works of his pupils that we can become acquainted with his system. After the intense labour which has been bestowed on bringing it forward,† it cannot be supposed to contain any errors, according to the strict notions of Werner; and if his pupils find it necessary to introduce any material alterations, and so to mould it, as to suit their own subsequent observations, it will no longer be the system of that philosopher,—which the arguments in the present paper are alone intended to meet.

The transition series is composed of limestone, graywacke, and graywacke-slate, trap and flinty-slate. Limestone is placed first, as being the oldest member, and is said to rest immediately on the newer clay-slate.‡ Of this we have no instance which I am acquainted with in Scotland, where, indeed, transition lime-

* Jameson's Mineralogy, vol. iii. p. 146.

† Werner, "after the most arduous and long-continued investigation, conducted with the most consummate address, discovered the general structure of the crust of the globe," &c. Jameson's Mineralogy, vol. iii. p. 42.

‡ Jameson's Mineralogy, vol. iii. p. 147.

stone may be considered as rather of rare occurrence. Graywacke and graywacke-slate are with us the principal members. The first of these is a stone usually of a bluish colour, passing into grey, and sometimes into greyish red; it is composed of fragments, often of considerable size, but sometimes so minute as to be scarcely distinguishable; these fragments are quartz, clay-slate, flinty-slate, and occasionally jasper, which are agglutinated by a basis of clay-slate, through which minute particles of mica are also sometimes dispersed.

Graywacke-slate differs from the fine grained graywacke only in its minute stratification and fissile character; it bears so strong a resemblance to clay-slate in hand specimens that even an experienced eye cannot distinguish it: in the rock it is not so easily mistaken; it usually alternates with graywacke, and is often remarkably contorted. Both substances are traversed by quartz veins, which are sometimes of enormous dimensions, but generally very minute and abundant.

The only limestones of this class that I know of are three: first, that of Rae Quarry, near Crook, in Peeblesshire, where it is interstratified with graywacke, and contains abundance of shells. The second is that of Cumberland, on the lakes of Windermere and Coniston, which also contains organised bodies. The third is the Plymouth limestone, which, according to the account of Professor Playfair, corroborated by Dr. Berger, is also transition limestone; and in it Mr. Playfair states that he found a petrified shell.* I have not myself visited the spot, but it is of consequence to observe that the limestones of all these different districts exhibit traces of organic remains. The other transition rocks are trap and flinty-slate; † but I have had no opportunity of observing either of them in their natural position. Such, according to Werner, is the extent of the transition series; but it does not comprehend all the rocks which occur in some of the transition districts, particularly that of Cumberland, although, with little exception, it is adapted to the south of Scotland in a very remarkable manner.

I may now notice the extent of country occupied by rocks of this description; but such is our limited acquaintance even with our own island that it can be done only in an imperfect manner. We know too little of the north of Scotland to be able to say what rocks occur beyond the Moray Frith; but it is by no means improbable, that when these regions have been more fully examined the transition series will be found among them. Indeed,

* Illustrations of the Huttonian Theory, p. 165.

† I suspect both these abound in the mountains of Cumberland, from specimens I have picked up among the loose fragments.

I have learnt from Dr. Macculloch that it occurs in great abundance in the north.

I am inclined to consider that it occupies a large proportion of Forfarshire; and if I be correct in an observation made on the banks of Loch Katrine several years ago, the transition rocks extend in that direction. I have likewise found traces of them on the right bank of the Clyde, near Dalnotter Hill, in Dunbartonshire. But the transition country we are best acquainted with is that of the south of Scotland, which stretches entirely across the island.

On the one side it begins near the boundary between East Lothian and Berwickshire, and continues along the coast to a little beyond the river Tweed. Extending a line from the first to a point on the west coast, between Girvan and Ballantrae; and from the second, another which shall pass by Langholm to a point between Annan and Carlisle, we shall find nearly the whole of the intermediate space to be transition, excepting where granite comes in, and some partial deposites of later strata, which occupy the lower parts of the valleys of Nith, Annan, &c. The mountainous district of Cumberland, Westmoreland, and the north of Lancashire, which is divided from the transition of the south of Scotland only by a small proportion of *parallel strata*,* belongs to the same, at least we know of none other with which it can be classed, although it contains a variety of rocks, which cannot be referred to any in the series of Werner. Adjoining to this, in the western part of Yorkshire, the same rocks occur: it is on these that the limestone of Ingleborough and Whernside rests. To this succeeds the extensive district of parallel strata, including the coal-fields of Warrington and Wigan, and the great alluvial deposite of Cheshire. These bring us to the neighbourhood of the Welch mountains, which I believe are all of the same nature, some specimens having been given me by a member of this Society, taken from the summit of Snowden. Graywacke, according to Mr. Aikin, makes its appearance at Church Stretton, in Shropshire; † and near Hay, on the border of Hereford, I observed it myself.

A great part of Somerset, and, finally, the whole of Devon and Cornwall, again excepting the granite, and a small portion of serpentine, and some other rocks, are all composed of transition strata. Thus, by extending a line almost due south, from Berwick to the English Channel, we shall find a large proportion

* This term has been applied to distinguish the sandstone strata, and in that sense I now use it; it is objectionable, however; for all stratified rocks present the phenomena of parallelism, consequently, without qualification, this term affords no distinction.

† Geological Transactions, vol. i. p. 212.

of the country to the west composed of transition rocks; while, so far as I know, none occurs to the east of it; although it is probable that at Mount Sorrel, in Leicestershire, some of the same series may be found.

We are still less acquainted with the precise limits of its extent in Ireland: we know, however, that it occupies the coast from Belfast Lough to the mountains of Morne, which are of granite; it also extends westward as far as Monaghan, and probably much beyond that point. From what Mr. Weld states, in his account of Killarney, it appears to be the principal rock of the Kerry mountains, and I know it occurs in great abundance in the county of Cork. Hence, even with the little information we possess respecting its exact limits, we have enough to know that the transition rocks form a very large proportion of the superficial extent of Great Britain and Ireland, and also comprehend the principal mining districts.

Having thus imperfectly chalked out the boundaries, or rather localities of the transition districts in these islands, I shall endeavour to show that some of the rocks of Cornwall are graywacke, in all respects similar to some of the south of Scotland; and if strata may be compared to the leaves of a book, a few decided and indisputable specimens are sufficient to characterise a district.

It was in consequence of some observations during a tour through Cornwall and Devon last summer that I was led to suspect this class stood in a very different relation in point of period, with respect to granite, from that which I had hitherto conceived; greater experience, or perhaps sufficient attention to the writings of Dr. Hutton, might have pointed out this before. Had I looked more attentively into his description of the granite district of Galloway, and at the same time attended to the nature of the stratified rock of which that country is principally composed, this fact would not have been new to me now. There were other circumstances, however, which severally contributed to prevent me from supposing that graywacke could occur in this position.

First, the unlimited use to which Dr. Hutton applied the term *alpine schistus,* left us quite uncertain with respect to the species of rock he meant: secondly, the alteration induced on graywacke, near its junction with granite,—a circumstance so strikingly exemplified in Galloway, that I own it deceived myself; and, lastly, the assertion I have so often heard repeated by the Wernerian geognosts, that granite veins never occurred excepting in rocks formed of the same constituents, alluding to gneiss and mica-slate.

Before I visited Cornwall, I knew that granite abounded in the Stannaries, and that tin and wolfram, metals which are considered nearly of the highest antiquity, were there common productions. I therefore expected to meet with a perfect epitome of the Wernerian system, containing the usual series of primitive rocks, descending from granite, through gneiss, mica-slate, and clay-slate, with all the *et cæteras* of serpentines, traps, and porphyries; but in this I was mistaken.

On my approach to Exeter through Somerset, I first observed the transition strata between Bridgewater and Taunton; and from thence traced them, more or less distinctly, till I crossed the river Teign, which bounds Dartmore on the east. Thus far great part of the country is very flat, some of it extremely hilly as a road, but none of it mountainous. The transition strata are by no means continuous, and in many places appear only in small projections above the surface.

On the right bank of the Teign the road winds up the side of a steep hill; and where the rock is cut there is a considerable display of strata, having all the external appearance of graywacke. On examining it, I found some of the strata coarser than others; but, in general, the grain was extremely fine, the texture solid and compact, the colour very dark grey: it was very tough under the hammer, it broke with a smooth and somewhat conchoidal fracture, and did not split into the thin laminæ of the graywacke-slate. This appearance puzzled me at first; the rock presented all the external characters of graywacke, and yet internally it was different. I had not proceeded many paces, however, when I came upon granite, the proximity of which, as before mentioned, is always marked by a very material alteration in the consistence of the adjoining rock. This alteration, I observe, was not unnoticed by Dr. Berger, in his interesting paper * on the physical structure of Devon and Cornwall. In mentioning graywacke, which he distinguishes from graywacke-slate only by its compactness, he says, "It is found higher up than the graywacke-slate, it may be supposed to have been precipitated more slowly, and under less powerful pressure; whereby the mass has been allowed to contract, and to assume a kind of crystallization. It rests immediately on granite." The conclusions he draws are different from mine; but from the above quotation it appears that the circumstance I observed at Teign Bridge is usual in similar situations all over Cornwall.

Near St. Austle, on the road leading to Carclaze mine, I found graywacke, in my opinion extremely well characterised; also on the road to Cambourn, not far from Dolcoath; likewise on the shore near Penzance. Here it is also fine-grained, and tough under the hammer, and at no great distance from granite.

* Geological Transactions, vol. i. p. 112.

Near Oakhampton I found it along with graywacke-slate, in the most unequivocal state; and on the shores of the Bristol Channel, near Ilfracombe, the rocks are all of the same material.* Here, on the beach, to the west of the town, I spent some hours the evening before I crossed to Swansea; and found nothing among the rocks to lead me for a moment to question that they were wholly composed of graywacke. Indeed I even remarked some of the contortions which are so common in this rock. Next morning, however, when walking down to the boat, under a point where a small battery is built, I found on the trodden surface of the rock an appearance very similar to mica-slate, for which substance it might readily be mistaken; but this resemblance appears to be owing to the friction of the feet, and the action of the weather, on a variety of graywacke, containing an unusual proportion of mica.

By casting an eye over the map of Cornwall, it will be observed that the above specimens are selected from the most remote corners of the peninsula. On examination I think they will be found sufficiently similar to the graywacke of Werner, to be entitled to be classed along with that rock. Dr. Berger, in his paper on the physical structure of Cornwall, gives them no other name, and if authority is to be qualified by experience, the opinion of one who has traced the footsteps of Saussure, and who has studied the geognosy of Werner with the utmost enthusiasm, cannot fail to be received with respect.

I have thus endeavoured to show by the selection of specimens, and by the opinion of a very scientific observer, that the stratified rock of Cornwall is graywacke. It would be uncandid, however, not to acknowledge that the *general* texture of this rock was different from the graywacke of the south of Scotland; it was more of the slaty variety, and frequently seemed, from its smooth and soft feel, to contain a large proportion of magnesian earth.†

I understand, in a course of lectures now delivering, a very material alteration has been proposed upon the Wernerian system, in order to introduce this rock in a position distinct, and very distant from graywacke. It appears to me much more simple to suppose that rocks of the same class in different districts may present peculiar characters, than that the operations

* The specimens alluded to were examined by the gentlemen present when this paper was read, who considered those from the road leading to Carclaze mine, and from near Oakhampton, as graywacke; and those from the vicinity of Penzance as greenstone.

† Since I read this paper I have had occasion to pass through the transition country of Peeblesshire, &c. On former occasions, I was in the habit of searching for characteristic specimens of the graywacke; I now looked for such as resembled the killas of Cornwall, which I found in abundance.

of nature should have been so multiplied and complicated, as to afford the endless distinctions which are thus required. Indeed, I cannot help thinking that if the killas of Cornwall had been sufficiently known it would have excluded entirely the introduction of that harsh-sounding German term graywacke. *Killas* appears to me to be as proper a translation of that word as specular iron ore is of eisen-glanz, and I think may be used with great propriety; distinguishing graywacke and graywacke-slate, by amorphous and schistose killas.

The only other rock of any importance in Cornwall is granite, termed *grauen* by the common people,—a name also given to clay-porphyry, a substance found pretty frequently in large veins. The shades of distinction chronicled by the mineralogist cannot be expected to attract the attention of the miner, who knows but two rocks, grauen and killas, throughout the Stannaries. It has been thought that a distinct rock was understood by the term *elvan*; but this is a mistake; elvan may sometimes be greenstone, but in general is either killas or granite, and is so termed by the miner when he finds the rock harder to work in one place than in another.

Before I entered Cornwall I was led to believe that it abounded in two kinds of granite, primary and secondary. Never having had an opportunity of comparing them *in situ*, I was anxious to do so here, and different localities were pointed out to me; these I examined with care, but could discover no grounds to justify any distinction. Dr. Berger makes no mention of secondary granite; and another gentleman, whose opinion on this, as on most subjects, will be received with the utmost deference, and who had the same object in view, during a visit made since I was there, informs me that he could discover no distinction at all. It is therefore of importance to ascertain whether the granite of Cornwall be new or old; which will easily be done by comparing the appearances it presents with the descriptions of these rocks as given in the Wernerian school; it is there taught that three formations of granite have been ascertained.

The oldest is the basis or nucleus, round which all other rocks have been deposited. The second occurs only in veins, traversing *only* the granite of the older formation. The third rests on some of the older primitive rocks, in an unconformable and overlying position. From this description of its external relations, it is evident that the granite of Cornwall can neither be the second nor third. With respect to its internal structure, we have the following definition: granite is a granular aggregated rock, composed of felspar, quartz, and mica. These alternate from large to small, and even to very fine granular. The large and coarse granular usually belong to the oldest; the small and fine granular to the newest granite formations. Besides felspar, quartz, and mica, other fossils sometimes occur in it; of these, schorl is the most frequent, then garnet and tinstone.*

At Penzance I observed some buildings constructed of a remarkably fine-grained granite; but this I no where saw *in situ*: otherwise, from Teign Bridge, where I first set my foot on granite, to the Land's End, it is generally of that character which entitles it to be ranked with the oldest variety. In many places it has suffered to a most wonderful extent by decomposition, but where it retains its freshness, no granite can possibly be better characterised. The specimens which I was able to bring away, and which are now before the Society, are by no means adequate to convey an idea of the coarse texture it sometimes presents. In the granite of Dartmoor the crystals of felspar are uncommonly large, often four inches in length. I believe it was from this neighbourhood that the flags of the foot-path on Westminster Bridge were brought; in these, crystals of felspar nearly as large may be observed. Granite countries usually present a bold and varied outline; but to this rule Cornwall is a most decided exception: its aspect is tame in the extreme, being comparatively flat,—a circumstance visibly occasioned by the corroding operations of time. Nowhere are the vestiges of degradation so remarkable as here. The enormous deposites of tin in the different stream-works, of which that of Carnon is perhaps the most extensive, clearly prove the destruction of surrounding mountains. This tin, in the shape of rounded pebbles, formed a stratum of about a foot thick, under a deposite of granite-gravel and mud, together forming an overburthen of 40 feet thick, and occupying a valley of very great extent. The lodes which furnished this tin must have existed above the level of the deposite; and from the quantity of metal deposited, they must have occupied a large tract of country. Other monuments of this general destruction may be found in the peaks which are seen in every direction in the granite districts of Cornwall. These are evidently the result of surrounding decomposition, and are formed of huge masses of rock, apparently piled on each other, with a regularity resembling masonry, and in all respects similar to the arrangement observable on the summit of every mountain in Arran, where the traces of time are also deeply furrowed.

Roach rock, a binary compound of quartz and hornblende, is another very remarkable instance of the same fact: this rock is flat at the top, and being quite perpendicular on three sides, when viewed from the west, presents the appearance of a square castellated building, which is rendered more conspicuous by being nearly of the same height as the tower of an adjoining

* Jameson's Mineralogy vol. iii. p. 102, &c.

church. There can be no doubt that this singular rock owes its present appearance to the operations of time on the surrounding materials, which its peculiar composition has enabled it to withstand.

The killas likewise presents marks of degradation where the country is composed of that rock. I noticed in some districts the roads mended entirely with quartz; the brilliant white appearance of which, after a shower, had a very curious effect. I could not comprehend by what industry the accumulated heaps of this substance were obtained: at last I perceived that they were gathered from the adjoining fields, and in some places picked from the surface of a common, by means of a hoe or mattock. That fragments of quartz should occur so unmixed with any others is only to be accounted for by supposing that they formed the quartz veins in the killas, which, from superior tenacity, resisted decomposition; while the softer parts of the rock, yielding to the action of the weather, were reduced and carried away.

We thus find that the granite of Cornwall possesses the characters ascribed by Werner to that of the highest antiquity. Some inferences may likewise be drawn in corroboration of its title to be classed with rocks of this description, from the nature of the metallic veins by which it is traversed. In the German account of the relative ages of metals, tin is the third, and wolfram the fourth, in order of antiquity.* If veins containing these metals be considered in other countries as indicative of rocks of the oldest primitive formation, the same application must be made to those of Britain.

I may now ask, if this be not the oldest granite, where are we to find it? as it appears to me impossible that any substance can more decidedly concur with definition. In the Alps, Dr. Berger must have learnt what primitive granite meant; yet not a doubt escapes him of the Cornish being any thing else. Distinctions either do or do not exist: if they do, character must be attended to; if they do not, it is quite unnecessary to add the terms secondary and tertiary to a substance possessing every attribute of a primary variety, merely because the structure of an adjoining rock does not accord with a specific theory.

Graywacke, or, as I shall in future call it, killas, I have before noticed, is a rock composed of fragments more or less comminuted, which must have existed in another state before they assumed their present arrangement. Along with the strata formed of these, beds of limestone are found, containing indications of organic remains. These are not confined solely to the limestone, they occur also in the killas; a fact which may be

* Jameson's Mineralogy, vol. iii. p. 275.

witnessed at any time, either in the neighbourhood of Coniston,* or on the right bank of the Blackwater, a little below Fermoy, in the county of Cork. The formation of this class of rocks was therefore subsequent to the formation of living animals, whose existence is supposed to be proved by the occurrence of organic remains in the composition of the rock.

In Cornwall, in Westmoreland, in Galloway, and in the counties of Down and Derry, this rock lies directly on granite, —a circumstance which we should at first sight be inclined to consider as indicating its subsequent formation. This thought, however, vanishes the moment we contemplate the veins of granite by which it is traversed. Of these there are many examples; but the most striking are at the Louran in Galloway, and at St. Michael's Mount in Cornwall.

It is many years since Sir James Hall laid before this Society an account of his observations on the granite district of Galloway, of which the Louran forms a part; and to the persevering activity of that gentleman, we are indebted for the display of one of the most interesting exhibitions of granite veins that exists. The peculiarities observable in Galloway were first pointed out to me by him; and as he has so lately favoured the Society with a particular account of them, it leaves me nothing to say regarding that quarter.

At St. Michael's Mount the shooting of the veins from the great mass of granite is also most strikingly exemplified. They were here first noticed by Professor Playfair, who compares them, most aptly, to the ramifications of the vegetable root; † for, indeed, nothing can be more illustrative of the phenomenon as it is here exhibited.

It is to be observed that granite veins, particularly when extremely minute, usually differ in texture from the mass to which they belong. While the little peak of St. Michael's Mount maintains a similarity of character with all the rest of the Cornish granite, not only in point of internal structure, but with respect to the tin and copper veins which traverse it, as well as by the massive blocks, hewn by the corroding hand of time, which ornament its summit; the veins that set off from it gradually become finer as they recede, but still preserve the perfect character of the rock. The importance deservedly attached by Dr. Hutton to the phenomena of granite veins, gave rise to a variety of hypotheses among those who were inclined to consider this rock as the original deposite, who have accounted for their formation in different ways.

* Since I read this paper, I wrote to a friend at Coniston, requesting a few of these specimens, well characterised, might be sent me: some of which are deposited, along with the rest, in the cabinet of the Society.

† Illustrations of the Huttonian Theory, p. 318.

It was first stated that they were formed of newer granite, and if properly examined would be found to cut the old granite as well as the rock which rested on it. This opinion was once very strenuously supported in this country; but as facts would not bear it out it was abandoned. I find, however, in a recent publication, something similar to it maintained by de Luc, who asserts that the veins at St. Michael's Mount are not granite, but merely quartz, which traverses the granite as well as the stratified rock. I cannot comprehend how de Luc could have been so much deceived at this place, as simple inspection of the smallest specimen will prove that he was mistaken.

It was next said that the veins in question were not true veins, but such as are termed cotemporaneous. To support which it was boldly asserted that they never extended beyond the limits of such rocks as were composed of the same materials, gneiss, and mica-slate.

I trust it is now distinctly shown that they do extend beyond these limits, and likewise that they traverse rocks from which, by no method of reasoning, it can be supposed that they could possibly be formed by secretion.

The last opinion is that which has recently been brought forward by Dr. Berger.* After describing the granite veins of St. Michael's Mount, he proceeds to say, that they are simply elevations on the plane of the granite existing previous to its being covered by the stratified rock; that the spaces between them were filled up as the graywacke was deposited; and hence the abrasion of the surface brought to light a section which has merely an appearance of veins. Were the devotion of Dr. Berger to his master less conspicuous in his geological disquisitions, I should be inclined, on the above statement, to call his character as an observer in question, having passed over in silence the detached masses of killas, which he could not fail to observe included in the granite, and which the above hypothesis is as far from accounting for as either of those mentioned before.

I have only a few specimens to lay before the Society from the veins of St. Michael's Mount; but they are equally interesting and satisfactory. One exhibits a portion of the killas bounded on each side by granite; another, a portion of two granite veins traversing killas; and the third, a mass of killas included in the granite. Simple inspection is sufficient, in the first place, to show that the opinion of de Luc is groundless with respect to the substance of these veins. One of the specimens also contains two small veins of quartz, which are of the kind called cotemporaneous; these keep the direction of the seams of the

* Transactions of the Geological Society, vol. i. p. 147.

stratified rock, and are cut off by the granite in the same line without any interruption.

To the opinion of Dr. Berger they also offer some reply. If the graywacke had been deposited on the granite in the way he supposes, it is natural to conclude that it would have been arranged in lines parallel to the sides of the elevations, somewhat similar to the coating of bark on the trunk of a tree: but in place of this the seams of the killas are set at an angle of about 30°, to the planes of intersection with the granite; consequently, if deposited from a supernatant fluid, they have assumed a very different position from that which either mechanical or crystalline influence would have induced.

The hypothesis suggested to Dr. Hutton by the appearance of these veins meets every difficulty: they conveyed to him evidence of being derived from a source of the greatest violence; and also that nothing but liquid matter injected from below could have created the disturbance among the stratified rocks, so conspicuous when in contact with granite. As it is a self-evident position that a rock which is cut by a true vein must have existed in a solid state previous to the formation of that vein; so is it equally obvious that if the vein can be traced into an adjoining mass, of which it is found to be a part, that mass must stand in the same relation, in point of period, to the rock which contains the vein, as the vein itself does: as also that if pieces of one rock be found imbedded in another, the including rock must have been of subsequent formation to the included. No theory, however, but that of Dr. Hutton can account for these appearances: to nothing but force can the position be attributed, which the stratified rocks have assumed in the vicinity of the unstratified; and nothing but matter injected in a liquid state could possibly have formed the shoots which traverse from the great mass of granite perforating the stratified rock, and at the same time envelope detached fragments of that rock. As the idea of violence in these operations has been so frequently combated, I cannot refrain from noticing here a very striking mark of it I met with at Coul in Ross-shire, when visiting Sir George Mackenzie. There the strata of gneiss are much disturbed by the invasion of granite veins: near which, on the outside curvatures of some of them I perceived rents similar to what we might expect on bending a flattened mass of clay, nearly deprived of moisture. I am fortunately enabled to present to the Society specimens illustrative of this interesting fact.

In the theory of Dr. Hutton we find also some grounds to account for the diminution of grain in the substance of the veins. The same cause to which in a former paper I attributed the gradation in the texture of greenstone, may be supposed to have acted here. It does not, however, observe an equal constancy, some veins of granite being as coarse-grained as the mass to which they belong.

In a former part of this paper I had occasion to notice an alteration which appears to take place in the texture of killas when in the vicinity of granite. This circumstance was so remarkable in Galloway, at the Louran and other places, that I took the strata so situated for mica-slate, although I had observed no line of separation between it and the killas; I was forcibly struck with this at the moment, but having then no time to follow it up I was obliged to leave the country without any particular examination. It will be observed, by the specimens from St. Michael's Mount, that the killas there assumes the appearance of fine-grained gneiss. At Wasseldale Crag, between Kendal and Shap, I noticed a rock, in the immediate vicinity of granite, quite similar; and I am told that the texture of the strata near the granite of the mountains of Morne is altogether the same.

This alteration is always of a gradual nature; and is so imperceptible that it affords a good example of what might be understood by the German term *passage*, or transition from one species to another; this *passage*, even admitting the substance altered, is of too limited a nature to constitute a distinct and totally different rock. This alteration, if traced with attention, may lead to some very important results; but, without entering upon it at present, I shall content myself with recommending it to the notice of geologists, some of whom may consider it of too minute a nature to deserve attention. They may, however, rest assured that it is only by an accurate examination, and a faithful detail of such objects, that we can hope to arrive ultimately at truth, the only solid basis of philosophic inquiry.

I may be accused of generalising too much in the foregoing statement, on grounds so limited; it must be remembered, however, that I have purposely confined myself to the examples of the relations which exist within my own knowledge, between the transition rocks and granite. The same phenomena are familiar where gneiss and mica-slate come in contact with that rock; but as these strata are considered to be of a very different age, the facts which I might have cited, had my object been to prove the age of granite with respect to all other rocks, were unnecessary when my purpose was to point out the relative ages of killas and granite.

From what I have said I consider myself warranted in finishing this paper with the following conclusions:

The killas of Cornwall belongs to the transition series of Werner.

The granite of Cornwall is possessed of every character by which the oldest varieties are distinguished.

That granite, the nucleus round which Werner conceives all other rocks were deposited, is in some cases actually of a later date than the transition series, which comprehends strata containing shells; and that its subsequent formation is clearly evinced by the appearances at St. Michael's Mount.

Hence that the distinction of transition rocks is grounded on false conclusions.

And, finally, that Werner must make very material alterations on his present system, if he wishes to accommodate it to the phenomena so commonly presented in nature.

Article III.

On Vomiting. Being the Account of a Memoir of M. Magendie on Vomiting, read to the Imperial Institute of France on the 1st of March, 1813.

The Class charged M. M. Cuvier, Pinel, Humboldt, and myself, to give our opinion of a memoir on vomiting by M. Magendie, Doctor of Medicine, read at the meeting of the 25th of January last.

This memoir treats of a physiological truth which for a century and a half past has been alternately adopted and rejected, acknowledged and denied, established and forgotten, and which M. Magendie has at last founded on proofs so irrefragable that it is completely established, and must henceforth be considered as a point of doctrine beyond the reach of every objection.

How is vomiting performed, and what are the means employed by nature for that act, so apt to disturb the health, and in many cases so well adapted to re-establish it? Such is the question which occupied the indefatigable and ingenious author of the memoir of which we have to give an account. He has not considered it with reference to medical practice, convinced that in what way soever it is produced, its necessity, indications, and effects, must continue the same in cases of disease. He has treated it as a skilful physiologist and judicious experimenter; and if we cannot ascribe to him alone the idea and the entire solution, it is just to say that without him it would still have remained a problem undecided.

Nobody doubted till towards the middle of the 17th century that vomiting was produced by the simultaneous contraction of the muscular fibres of the stomach, supposed by anatomists to exist in that organ upon no very strong evidence. M. Magendie says, in his memoir, that Chirac appears to have been the first who entertained the contrary opinion, and who advanced that the diaphragm and abdominal muscles are the essential agents; but we have found that Bayle entertained the same opinion long before that physician, and that he confirmed it by experiments, which, if they were really made, must deprive Chirac of the priority, without injuring the proofs by which he confirmed his opinion. Senac informs us that Bayle, having caused a dog to swallow an emetic, made a deep incision opposite to the stomach, through which he introduced his finger while the animal was in the act of vomiting, and found by repeated trials that the stomach was not in motion. He found that the whole action was produced by the diaphragm and abdominal muscles, the most powerful of which, according to Senac, are the two transverse muscles, the only ones which have a semicircular direction, and which are capable of forming those hollows that appear in the belly in the act of vomiting. It is needless at present to discuss this subject.

The system of Bayle, or of Chirac, had its partisans; but it met likewise with opponents. These indeed could not but be numerous at a time when it was believed that the food was triturated in the human stomach in the same way as it is in the gizzards of birds.

On this occasion there occurred a pretty keen discussion between two members of the Academy of Sciences, Litre and Duverney; one of whom employed inaccurate reasoning, the other inconclusive experiments; and neither was able either to convince the followers of Chirac, or persuade his antagonists. Lieutaud and Haller, almost at the same time, put themselves at the head of the last party. They endeavoured to prove that vomiting is exclusively performed by the stomach, and that it is independent of the diaphragm and abdominal muscles, which in their opinion only concur accidentally with the action of the stomach. Lieutaud observed that the action of the diaphragm and abdominal muscles being subject to the will, vomiting ought to be voluntary if it was occasioned by any such action; yet this is the case only in a small number of instances. Haller opposed the opinion of Chirac in order to strengthen his own system of *irritability*, under which he wished to arrange all the phenomena of animal organisation.

Wepfer took the same side, and he deceived himself still more than his predecessors; for he had recourse to experiments, and was misled by the results. He employed poisons by way of emetics, which excited in the stomach, sometimes in its place, sometimes out of the body, movements which he considered as muscular actions, though they were only the effect of that contraction which takes place in living substances when attacked by corrosives.

The high reputation of Haller, and the influence of his works, almost effaced the very recollection of the true notions of vomiting which had been occasionally perceived; and during 50 years it has been uniformly taught and believed that vomiting is produced by the stomach alone, till M. Magendie turned his attention to the subject, and resolved to subject it to experiments so rigid, and so often repeated, as to put the question beyond dispute, and render the conclusions classical, both in books and the schools.*

We think proper to mention here that M. Richerand, an esteemed professor and author, convinced by facts exhibited before him, as well as before us, has introduced them into his treatise of Physiology, and has employed them to explain the notion of vomiting which he has embraced.

It is principally by the faithful account of these facts that M. Magendie so greatly interested the Class, already accustomed to esteem his talents and appreciate his discoveries. It is by recalling them to our colleagues that we hope to interest them in our turn.

We have not to notice simple conjectures, or slight and trifling attempts, from which systems have been too often built, and opinions formed respecting the most difficult points. Never, perhaps, were experiments more multiplied on the same object, or more scrupulously conducted, or with more exactness. They have been repeatedly made before us. We carried to them a considerable portion of doubt, perhaps even of incredulity, without, however, suspecting the well-known veracity of their author. We have seen, examined, handled, and we declare that our conviction is full and complete.

All the experiments which we witnessed were made upon dogs, because they are the animals most subject to vomiting. Tartar emetic was almost always employed to produce vomiting, not by way of injection or deglutition, but by introducing it into the jugular vein, as is done by the veterinary schools of Denmark. And it is worthy of remark, that tartar emetic, when swallowed by the animal, often does not occasion vomiting in half an hour; but when introduced directly into the circulation, it produces vomiting in one or two minutes. We have reason to be astonished at this constant and irresistible tendency of tartar emetic to produce vomiting, so that wheresoever it is applied it always produces this effect.

* Note by the Editor.—The preceding historical detail is not so accurate as the French author conceived; nor is the claim of M. Magendie quite so incontestible as he imagined. The same opinions were taught by Mr. John Hunter more than 20 years ago. In his *Observations on Digestion* he says, that vomiting is performed entirely by the diaphragm and abdominal muscles; that it is not necessary for the stomach to act at all, and that vomiting is to the stomach what coughing is to the lungs. See his Observations on certain Parts of the Animal Œconomy, p. 199. 2d Edition; London, 1792.

As Bayle, Chirac, and Duverney had announced, M. Magendie made us perceive by the touch that during the act of vomiting the stomach remains in a state of inactivity, and that it is the diaphragm and abdominal muscles which produce the evacuation of that organ. During this first experiment, repeated several times upon large dogs in the abdomen, of which an incision had been made large enough to admit two fingers, we perceived that at each strain of the animal our fingers were pressed upon from above by the liver pushed down by the diaphragm, and from below by the intestines which the abdominal muscles pressed, while the stomach, emptying itself without any sensible motion, did not appear to diminish in volume. This last singularity, already observed and announced to the Class by M. Magendie, is occasioned by the presence of air, which takes the place of the food as it is thrown out of the stomach, and which, being introduced through the œsophagus during the long inspirations which precede vomiting, keeps the stomach always sufficiently distended not to escape the compressing action of the surrounding parts.

We know that it is easy to swallow air. Some people amuse themselves by swallowing it, and thus swell out their stomach till it resounds like a drum when struck. There can be no doubt that a great deal of air is swallowed during vomiting, without which vomiting would be exceedingly painful, as happens in cases of poison by corrosive substances, when the stomach is contracted, and no longer capable of admitting that fluid. M. Magendie intends soon to read a memoir on this subject to the Class, and we ought not to anticipitate what has become his legitimate property. We shall observe the same silence with respect to the conspicuous part which the œsophagus takes in the act of vomiting, because M. Magendie is drawing up a memoir on the subject.

In a second experiment, made upon the same dogs which had served for the preceding, the incision of the belly being increased, and the stomach drawn out of the body, it was still easier for us to be convinced of its want of motion, and to perceive the inaccuracy of what Haller had advanced respecting its peristaltic movement. In this state the stomach, filled with air which had been drawn in some moments before the act of vomiting, was distended like a balloon; but no farther vomiting took place, nothing but ineffectual nauseas, because the stomach being out of its place could no longer be acted upon by the surrounding organs.

M. Magendie announced in his memoir that by pressing upon the stomach thus removed out of the body with the two hands, so as to imitate in some measure the action of the diaphragm and abdominal muscles, vomiting was always produced. And this constitutes one of the most conclusive arguments in favour of the opinion which he embraced; but though the dog subjected to this experiment vomited without having taken any emetic, and exhibited the nausea, and other symptoms which characterise vomiting, the column of air did not enter and take the place of the ejected food. This shows us that other conditions besides the mere pressure of the stomach are necessary to produce vomiting. This experiment revealed to M. Magendie the principal of these conditions. When he held the stomach in his hands without compressing it, he perceived that when he drew it too far out of the belly he immediately produced nausea and vomiting. He conceived that it was the stretching of the œsophagus which produced this double effect; and he took advantage of this discovery to make dogs vomit at pleasure which had taken no emetic, or to hasten vomiting when the emetic did not act with sufficient promptness. It was only necessary in either case to agitate the stomach, and draw the œsophagus a little, to produce immediate vomiting. It is easy to perceive here the effect of those profound inspirations which, as well as nausea, precede vomiting, and by means of which, the diaphragm embracing the œsophagus between its pillars, draws it along with it towards the intestines, and makes it undergo those tractions which M. Magendie has so happily imitated. This explains why in the palsy of the œsophagus there is no vomiting, and why it is so difficult to produce it after cutting the pneumogastric nerves.

If we examine a person just going to vomit, if he does not succeed after a strong inspiration, we see him repeat it again and again, and multiply the movements of expiration, which are always more irregular. By this means the diaphragm agitated up and down gives to the œsophagus that agitation without which, in all probability, vomiting would not be produced.

It is well known that vomiting often takes place without all those efforts. This is an objection which may be started against either opinion. But, besides that we do not speak of those individuals, who, from the frequent practice of vomiting, have acquired the habit of it, we must distinguish, in infants at the breast, for example, the regurgitation of vomiting; and, in persons who ruminate, the voluntary and tranquil act of bringing from the stomach to the mouth the food to be swallowed a second time, from the painful and involuntary act of vomiting. Besides, in persons who ruminate, as has lately been observed by one of your commissioners in a young man of 24 years of age, the return of the food to the mouth is preceded by a kind of noise, sometimes pretty loud, which announces the instantaneous agitation of the œsophagus, produced by the diaphragm, and the no less prompt action of the œsophagus upon the stomach.

This agitation of the œsophagus is not confined to the alimentary canal, properly speaking; the branches of the par vagum, and of the great intercostals which cross around it, must participate in this movement.

We observed above that as long as the stomach of dogs labouring under an emetic was out of the body, no vomiting took place, but only nausea; but when the stomach was restored to its place, vomiting immediately followed. The next point to be determined was, if the action of the abdominal muscles be absolutely necessary to produce vomiting, as was the opinion of Chirac and his adherents. These muscles were removed from a robust dog, and an emetic being injected, he vomited apparently with as much facility as if that operation had not been performed, which reduced the covering of the abdomen to the peritoneum, and to a few transverse muscular fibres which it was impossible to remove. M. Magendie made us remark in this case the great tension of the linea alba, during the nausea and vomiting; and we conceive that this species of cord stretched along the abdomen may be sufficient to keep the intestines in their places, and to prevent them from escaping from the energetic action of the diaphragm, which in some of the experiments even tore the peritoneum in several places.

One of us had made an analogous observation, but without drawing the same consequence, upon a soldier, the muscles of whose abdomen had been removed or destroyed by the action of a large cannon ball; so that after his cure the stomach in all its positions might be seen through the transparent peritoneum. This soldier, during his cure, was frequently troubled with vomiting, to which the abdominal muscles could not contribute, as they were wanting altogether; yet he vomited with as little difficulty as before his wound.

The experiment above related, which was first thought of by M. Magendie, proves that it is the diaphragm which acts with the greatest efficacy in vomiting, and that the abdominal muscles serve scarcely any other purpose than to confine the viscera floating in the abdomen, and to oblige them to re-act in a contrary direction. But when the action of the diaphragm is carried too far, and when the inspirations are too profound and too long, then instead of vomiting we have alvine evacuations, doubtless because the œsophagus is too much pressed upon by the diaphragm to give free passage to the substances which endeavour to escape from the stomach.

When, on the contrary, the diaphragm can only act feebly, and solely for the maintenance of respiration, as happens when the phrenic nerves are cut, then how strong soever an emetic is administered, nothing more takes place than successive nauseas, and very seldom vomiting, notwithstanding the most violent contractions of the abdominal muscles.

One of the commissioners having invited M. Magendie to cut the phrenic nerves on both sides of a dog still vigorous, whose abdominal muscles had been removed, and to make him swallow a gros (72 grains) of red oxide of mercury, the animal was very much agitated, and had nauseas, retchings, and painful alvine evacuations, but did not vomit. M. Magendie intends speedily to state the observations which he made on this occasion.

Most of these experiments prove sufficiently that the stomach is entirely passive in the act of vomiting, and that the principal effect is produced by the diaphragm. Those that follow go still farther, since they demonstrate that vomiting may take place without the stomach. They were repeated three times in our presence, with the same result.

M. Magendie having cautiously (in order to avoid hemorrhages) made a ligature on each of the orifices of the stomach, removed that viscus altogether, and, after having sewed up the wound in the belly, administered an emetic. In less than two minutes the dog exhibited all the symptoms which precede vomiting. We may even say that he actually vomited, for he threw out with effort and violent nausea the mucus of the œsophagus. Thus it appears that vomiting may in some measure take place without the stomach. It appears, then, that as far as vomiting is concerned the stomach is nothing but an inert bag, containing matters destined to be thrown out. And what other part in vomiting is it possible to ascribe to those schirrous stomachs whose coats have acquired some inches of thickness, and a hardness approaching to that of cartilage?

We have only another experiment to notice, and it is the most extraordinary and the most decisive of all those which we have seen.

In the place of the stomach, which had been cut out of several dogs, M. Magendie substituted a small hog's bladder, almost of equal capacity, to the neck of which a canula of caoutchouc had been adapted, which was thrust into the œsophagus below the diaphragm, and kept in its place by a thread. These dogs were made to swallow water tinged yellow, with which the bladder was filled according as deglutition took place. The opening of the belly having been sewed up, an emetic was introduced into the jugulars. Nausea took place in a short time, and the animals vomited the yellow water precisely as if it had come from a real and living stomach. The wound in the belly being laid open, we easily observed at each strain the air descending in a current into the bladder, and distending it as if it had been a real stomach, which is not the least curious circumstance attending this experiment.

It only remains for us to submit to the Class some reflections which M. Magendie did not think it necessary to add to his memoir, though he did not fail to make them as well as ourselves, on the question whose destiny he has thus finally fixed.

These experiments prove not only that the stomach is passive in vomiting, they lead us to a more important result, which throws new light upon the nervous energy, that wonderful energy which constitutes the whole of our being, the mysteries of which it is so much our interest to penetrate. We may deduce from the result of these experiments that the principle, the prime mover of all those movements which produce vomiting, has its source in the seat of the nervous energy itself; for we cannot otherwise explain how an emetic, which produces no action on the stomach, determines the contraction of the diaphragm and abdominal muscles. We cannot have recourse here to those sympathies which have been so much abused in physiology, by advancing that the contraction of the stomach draws along with it by sympathy that of the muscles just mentioned. It is obvious that an emetic can only produce its effect by reacting from the stomach upon that place of the seat of the nervous energy, where the principle of the contraction of the diaphragm and abdominal muscles resides. It is the affection of that part which is the immediate cause of vomiting. If the nerves, by which the diaphragm and the abdominal muscles receive the impression of it, were cut, the patient would have the same desire to vomit, and would have the sensation of vomiting without vomiting in reality. This is proved by the suspension of vomiting in M. Magendie's experiments on cutting the phrenic nerves. On the other hand, though these nerves, and all the rest of the body, remained untouched, if that portion of the seat of the nervous energy were disorganised, no emetic could give the animal either a desire to vomit, or produce in him the sensation of vomiting.

We have here a particular and very remarkable application of that general truth demonstrated by M. le Gallois, namely, that the seat of the nervous energy (the brain and spinal marrow) is the sole source of all the motions which take place in a living animal, and that no part can move without a particular and anterior modification of that part of the nervous energy by which it is animated. The obstinate vomiting which in many cases accompanies apoplexy, and which had been ascribed to indigestion, had been already pointed out by M. le Gallois as a phenomenon entirely unconnected with every affection of the stomach, and totally depending upon that of the brain.

It remains to be known how an emetic introduced into the stomach can affect the seat of the nervous energy in a manner so as specifically to produce vomiting. Is it by irritating the nerves of the stomach? Or is it absorbed, introduced into the blood, and transported by the circulation? Perhaps both of these modes of transmission take place, according to circumstances. The vomiting which we observe sometimes after cutting the nerves of the eighth pair, and which appears to be occasioned by the irritation which the superior segment of these nerves experiences, seems to favour the first mode of action; while the experiments of M. Magendie producing vomiting even in animals deprived of their stomach, by injecting an emetic into the blood-vessels, seems equally favourable to the second mode. His preceding experiments on the effect of upas, experiments made in concert with M. Delille, strengthen this last opinion. They prove that upas occasions those dreadful convulsions which so speedily destroy life, only when it is absorbed into the mass of the blood, and transported directly to the spinal marrow. It is very probable that almost all substances that have some effect on the animal œconomy act in this manner. This opinion leads us to views entirely new respecting the mode of action of most medicines and poisons.

Another question remaining to be answered, is to know the precise part of the brain or spinal marrow on which the efforts of vomiting depend. M. le Gallois has proved that the principle of the movement of inspiration is seated in that portion of the medulla oblongata which gives origin to the eighth pair of nerves. If we consider that the efforts of vomiting are executed by the muscles of respiration, that the nerves of the eighth pair supply the stomach as well as lungs, and that the disorder of the medulla oblongata in apoplexy occasions vomiting, it will be rendered pretty probable that the efforts of vomiting are situated not far from those of respiration, if they have not the very same position. But it would be of importance to determine the point by direct experiments. Now that the general seat of the nervous energy is well determined, and clearly defined, one of the greatest objects of physiology is to know precisely the function peculiar to the different portions of that seat. Such objects deserve the attention of such accurate experimenters as M. M. le Gallois and Magendie; and those experiments, which they have already made so successfully, induce us to hope that they will advance still farther in a career in which they know by experience that they are likely to meet with honour, glory, and reputation.

To conclude, we think, 1. That M. Magendie, to whom the Class has already given with so much pleasure proofs of its esteem and satisfaction for the experiments previously communicated, deserves new ones for those which he has just presented.

2. That his memoir on vomiting, destined to be ever after cited in physiological works, is worthy in the first place of being mentioned in the history of the labours of the Class, and of an honourable place in its memoirs.

3. That M. Magendie ought to be invited by the President to give to his experiments the farther developements of which they are susceptible; and to demand, if he thinks proper, a reimbursement of the expenses which he may have incurred, or may still incur, in the further prosecution of the subject; for we expect that he will examine with particular attention the phenomena of vomiting in birds, and other animals destitute of a diaphragm.

(Signed) Cuvier, Pinel, Humboldt, Percy, *Reporter.*

The Class approves of this Report, and adopts its conclusions.

Certified conformable to the original.

The Perpetual Secretary, Knight of the Empire,

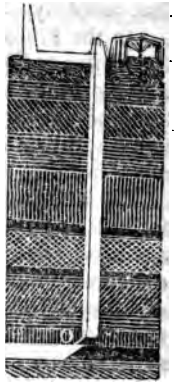
G. Cuvier.

Article IV.

An Account of the dreadful Accident which happened at Felling Colliery, near Sunderland, on May 25th, 1812. Extracted from an introductory account prefixed to The Funeral Sermon preached on the occasion, and published, by the Rev. John Hodgson.

(*Concluded from* No. V. p. 365.)

On the 1st of June, one of the ropes of the scaffold gave way, and on the next day, about five o'clock in the afternoon, the whole of it fell to the bottom of the pit. Immediately after this a second scaffold was suspended; but when eight fothers of clay had been thrown upon it, it also broke its ropes and fell to the bottom, about eight o'olock on the evening of the same day. At ten o'clock another expedient was resorted to: three beams of timber were laid across the mouth of the shaft, a little below the surface, and these were traversed with strong planks, upon which, on that evening, and early next morning, a body of clay was laid four feet thick, and firmly beaten together. At the same time a ten inch stopping of brick and lime was put into the tube drift of this shaft: this drift had long been closed, but the additional stopping was added, for greater security against the fire-damp escaping.



Preparations now began to be made for re-opening the mine. For this purpose a brattice or partition of thin deals, began to be put down the William Pit; of which and its furnace-tube and whim-gin, the annexed figure is a section. The black line down the shaft represents the brattice, which, in this case, was made to assist the workmen in raising the clay thrown down the shaft on the 27th and 29th of May.

About this time many idle tales were circulated through the country concerning several of the men finding their way to the shafts, and being recovered. Their number was circumstantially told—how they subsisted on candles, oats, and beans—how they heard the persons, who visited the mine on the day of the accident, and the Wednesday following, but were too feeble to speak sufficiently loud to make themselves heard. Some conjurer, too, it was said, had set his spells and divinations to work, and penetrated the whole secrets of the mine. He had discovered one famishing group receiving drops of water from the roof of the mine—another eating their shoes and clothes, and other such pictures of misery. These inventions were carefully related to the widows, and answered the purpose of every day harrowing up their sorrows afresh. Indeed, it seemed the chief employment of some to make a kind of insane sport of their own and their neighbours' calamity.

On the 19th of June, it was discovered that the water oozing out of the tubbing of the William Pit, had risen to the height of 24 feet upon the clay. On the 3d of July, this being all overcome, the brattice finished, and a great part of the clay drawn up, the sinkers began to bore a crow-hole at O, out of the shaft into the north drift. On the next day, the stoppings in the tube drift of the John Pit were taken down, and the bore-hole finished, through which the air passed briskly into the mine, and ascended by the John Pit tube.

Some experiments made on the fire-damp, by collecting it in bladders in the John Pit tube, before the bore-hole was opened, proved that it would not ignite previous to its mixture with atmospheric air. This shaft became an up-cast at three in the afternoon of the 5th of July; at seven on the same day, the fire-damp exploded on its being exposed to the flame of a candle. From the 6th to the 8th, it continued in the same state, and after that became so saturated with atmospheric air, as to lose that property.

On the 7th of July, the workmen pierced through the clay in the William Pit into the drift; and at 45 minutes past eleven in the morning, the John Pit tube emitted a thick continued volume of vapour, alternately of a blackish and a grey colour: at five in the afternoon, it was of a light steam colour, and the next morning scarcely visible.

The morning of Wednesday the 8th of July, being appointed for entering the workings, the distress of the neighbourhood was again renewed at an early hour. A great concourse of people collected—some out of curiosity—to witness the commencement of an undertaking full of sadness and danger—some to stir up the revenge and aggravate the sorrows of the relatives of the sufferers, by calumnies and reproaches, published for the sole purpose of mischief; but the greater part came with broken hearts and streaming eyes, in expectation of seeing a father, a husband, or son, "brought up out of the horrible pit!"

As the weather was warm, and it was desirable that as much air might pass down the shaft as possible, constables were placed at proper distances, to keep off the crowd. Two surgeons were also in attendance, in case of accidents.

At six o'clock in the morning, Mr. Straker, Mr. Anderson, the overman of the colliery, and six other persons, descended the William Pit, and began to traverse the north drift towards the plane board. As a current of water had been constantly diverted down this shaft for the space of ten hours, the air was found to be perfectly cool and wholesome. Light was procured from steel-mills. As the explosion had occasioned several *falls* of large masses of stone from the roof, their progress was considerably delayed by removing them. After the plane-board was reached, a stopping was put across it on the right hand, and one across the wall opposite the drift. The air, therefore, passed to the left, and number *six* was found.

The *shifts* of men employed in this doleful and unwholesome work, were generally about eight in number. They were four hours in and eight hours out of the mine: each individual, therefore, wrought two shifts every 24 hours.

When the body of number six was to be lifted into a shell or coffin, the men for a while stood over it in speechless horror: they imagined it was in so putrid a state, that it would fall asunder by lifting. At length they began to encourage each other "in the name of God" to begin; and after several hesitations and resolutions, and covering their hands with oakum to avoid any unpleasant sensation from touching the body, they laid it in a coffin, which was conveyed to the shaft in a bier made for the purpose, and drawn 'to bank' in a net made of strong cords.

It is worthy of remark that number six was found within two or three yards of the place where the atmospheric current concentrated, as it passed from the one pit to the other; but that he was lying on his face with his head downwards, apparently in the position into which he had been thrown by the blast. The air visited him in vain.

When the first shift of men came up, at ten o'clock, a message was sent for a number of coffins to be in readiness, at the pit. These being at the joiner's shop, piled up in a heap, to the number of 92, (a most gloomy sight) had to pass by the village of Low Felling. As soon as a cart load of them was seen, the howlings of the women, who had hitherto continued in their houses, but now began to assemble about their doors, came on the breeze in slow fitful gusts, which presaged a scene of much distress and confusion being soon exhibited near the pit; but happily, by representing to them the shocking appearance of the body that had been found, and the ill effects upon their own bodies and minds, likely to ensue from suffering themselves to be hurried away by such violent convulsions of grief, they either returned to their houses, or continued in silence in the neighbourhood of the pit.

Every family had made provision for the entertainment of their neighbours on the day the bodies of their friends were recovered; and it had been generally given out that they intended to take the bodies into their own houses. But Dr. Ramsay having given his opinion that such a proceeding, if carried into effect, might spread putrid fever through the neighbourhood, and the first body, when exposed to observation, having a most horrid and corrupt appearance, they readily consented to have them interred immediately after they were found. Permission, however, was given to let the hearse, in its way to the chapel yard, pass by the door of the deceased.

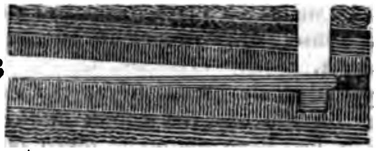
From the 8th of July to the 19th of September, the heart-rending scene of mothers and widows examining the putrid bodies of their sons and husbands, for marks by which to identify them, was almost daily renewed; but very few of them were known by any personal mark—they were too much mangled and scorched to retain any of their features. Their clothes, tobacco-boxes, shoes, and the like, were, therefore, the only indexes by which they could be recognised.

After finding numbers seven, eight, and nine, the operations of the first day ceased, about ten o'clock in the evening. At six the next morning the workmen began to put deal stoppings into the stentings of the double head-ways west of the William Pit. In the afternoon number ten was found, and the third board south of the plane-board discovered to be much fallen: carrying a brattice nearly to its face was the last proceeding of the 9th.

Early in the morning of the 10th of July the air in the William Pit was discovered to be casting up with a current so feeble as nearly to approach to stagnation. This being supposed to be caused by the water collected about the bottom of the John Pit approaching the roof of the mine, the machine was put in readiness for drawing it. A collection of water amounting to about 4500 gallons was twice a week raised from a *sump* or well, immediately under the John Pit shaft. This sump was made for the purpose of receiving it as it oozed from the tubbing. The dip of this colliery being about one yard in twelve to the south-west, the lowest part of the colliery was consequently at this shaft, and the little water that the mine produced collected here. The double head-way was nearly water level. The annexed section may assist in giving a clear idea of the appearance of the water when the circulation of air through the mine began to stop. A represents the shaft, and B the inner narrow-board.

A

B



Hitherto the air had descended into the mine by the John Pit tube: but now the clay laid over the mouth of this pit on the evening of the 1st of June was removed, and the *settle boards*, or frames, upon which the corves are loaded, were refixed. At 45 minutes after four o'clock this afternoon the water began to be drawn in buckets, each containing 90 gallons: 30 buckets were drawn in an hour.

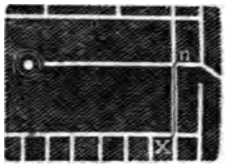
On the morning of the 11th a larger stream of water than had been hitherto used was diverted down the William Pit, with the expectation of forcing the air to descend with it. This was a desirable point to effect, as the bodies of the sufferers might be more readily obtained by this pit than the other; but as the water fell about the John Pit the atmospheric current set more strongly down it: the attempt was therefore abandoned as hopeless.

The machine was constantly at work drawing water till Monday the 13th, when the rubbish occasioned by the falling of the two scaffolds on the 1st of June, stones blown from the roof by the blast, and the body of a horse, began to be raised. As the body of the boy number 11 had lain a long time in water it was perfectly white, On Tuesday the 14th of July, as the workmen were clearing out the water-sump at the bottom of the John Pit, a gust of fire-damp burst from the workings, and ascended the shaft. This caused so great an alarm that the cry "Send away a loop!" from the bottom, and "Ride away! Ride away!" from the banksmen, were heard together. Seven of the men clung to the rope, and arrived safe at bank; and two old men threw themselves flat upon their faces, in expectation of an explosion; but, after a second and similar eructation, the atmospheric current took its usual course. No alteration was perceived at the William Pit. This phenomenon was afterwards ascertained to proceed from a large fall at that time taking place in the stable board, and forcing back a foul admixture of the two damps and common air. The banksmen's cry so alarmed the villages of High and Low Felling that all the inhabitants, young and old, hastened to the pit. At two o'clock in the afternoon the work was resumed.

On the 15th of July the bottom of the plane-board was reached, where the body of a mangled horse and four waggons were found. Though these waggons were made of strong frames of oak, strengthened with hoops and bars of iron, yet the blast had driven both them and the horse with such violence down the inclined plane-board that it had twisted and shattered them as if they had been shot from a mortar against a rock. Number 12, though a putter, at the time of the accident was employed at the meetings of the inclined plane, to keep the ropes in order as the waggons passed each other. Number 13, from the position in which he was found, seemed as if he had been asleep when the explosion happened, and had never after opened his eyes. He was seen about a quarter before eleven o'clock smoking his pipe on the place where his body was found. He attended to the five horses, and had the charge of keeping the waggon and inclined plane ways free from obstructions.

After obtaining number 14 the crane was visited. Here 21 bodies, from number 15 to 36, lay in ghastly confusion: some like mummies, scorched as dry as if they had been baked. One wanted its head, another an arm. The scene was truly frightful. The power of the fire was visible upon them all; but its effects were extremely various: while some were almost torn to pieces there were others who appeared as if they had sunk down overpowered with sleep.

From an apprehension that the great body of fire-damp confined by the stoppings newly put into the walks immediately south of the plane-board, might burst forth if kept perfectly tight, the atmospheric air was thrown into the full-way board by a stopping placed across the plane-board, a little above the crane. As soon as numbers 42, 43, and 44, were coffined, the air was conducted to number 45. After this the stopping above the crane was taken down, and the workmen were employed from the night of the 18th to the morning of the 22d of July, in making a brattice from the north-west corner of the fourth right-hand pillar above the crane, to the south-east corner of the pillar next above the drift to the William Pit. By this contrivance the fire-damp on the south side of the plane-board was not only pent in by two rows of stoppings above the crane, but it was left at liberty to escape into the drift on the south side of the brattice, represented by the line x n in the annexed figure.



July the 22d. Numbers 46 and 47, as well as 39, had probably attempted to make their escape from the blast—they were lying on their faces, their heads downwards, and their hands spread forwards: 46 was working with 48; and 39, 47, 49, and 50, were blasting stone from the roof at 49.

Little progress was made on the 23d; for after 51 was found, the day was chiefly spent in removing two heavy falls under which 52 and 53 were buried. The last of these had his employment in the second board south of the plane-board; he had therefore at the time of the accident either not commenced his work, or left it to talk with the young men at 49.

About ten o'clock this evening the piece of solid coal between the face of the first board south of the William Pit, and the double head-ways on the west of it, began to be pierced. After being bored through with a miner's auger, the hole was kept perfectly tight by a wooden plug, while a passage for the men was opened. Iron picks were used till the coal was thin, when it was battered down in the dark with a wooden prop. Then picks of oak and lignum vitæ, hardened in the fire, were used in widening the avenue; and the steel-mills not suffered to play till the air took a regular suck past 54, 79, 78, and behind the brattice, x n into the William Pit drift. This work was finished a little after twelve o'clock.

Before two o'clock in the morning of the 24th number 54 was reached. It is worthy of remark that nearly the whole of the men found in this line of boards had fallen on the very spot where they were employed. In the progress of obtaining the bodies from 54 to 60, nothing particular occurred except a large fall, under which number 59 was found.

On the 25th of July, 11 bodies, from 61 to 71, were interred. Number 64 was under a large fall. This man was keeper of the Heworth poor-house, and a class leader of the Wesleyan sect of Methodists. A pamphlet has been published, containing 24 pages, and entitled "A short Account of the Life and Christian Experience of John Thomson, &c. compiled chiefly from his own Journal. By Theophilus Lessey. Newcastle-upon-Tyne, printed by J. Marshall, 1812. The profits of this pamphlet will be faithfully applied to the relief of his widow, and five orphan children."

The boards of 59 and 64 were the only ones fallen in this sheth: each board here was bratticed nearly to its face, more with a view of rendering them pure and clean, than of giving assistance in obtaining the bodies; for the workmen, out of anxiety to recover them, became fearless of danger, and ventured into the repositories of foul vapours before the brattice was long enough to convey sufficient atmospheric air into them to render them wholesome. The 26th of July, being Sunday, was a day of rest.

On the 27th of July seven bodies were obtained: 72 and 73 were much burnt, but not much mangled: 74, 75, 76, and 77, were found buried amongst a confused wreck of broken brattices, trap-doors, trams, and corves, with their legs broken, or their bodies otherwise miserably scorched and lacerated. Before 78 was found the brattice represented in the last figure was taken down; a stopping put across the plane-board at number 41; and the air thrown past 79 and 54 through the aperture (which had been partly made by battering down the coal with a prop) and thence into the William Pit. This wall, on account of the prevalence of fire-damp, when 45 was found, had not been crossed till now.

The 28th of July was chiefly spent in putting up stoppings along the wall, from 78 to 79. Number 80 had been blown through a stopping.

Numbers 81 and 82, the latter under a fall, were found on the 29th of July.

On the 30th of July the fall, which commenced a little east of 82, was found to continue, and 83 and 84 were dug from beneath it: 85 kept the sheth down-going door opposite the William Pit on the east: his hair, which was of a light colour, had been burned off; but had grown again to the length of an inch or more.

As all the upper parts of the mine in which there was a likelihood of meeting with any bodies had been once carefully gone over, and it was known that three persons had not escaped from the newly formed boards on the south-east, the air, on the 31st of July, was diverted, and thrown up the head-ways from the plane-board. Number 86 perished by the first explosion; for as H. Anderson escaped he felt his body under his feet; but having a living boy in his arms he was unable to bring him out. He was employed in driving a waggon from the south crane at number 88. His horse, which was lying near him, had been turned round and thrown upon its back, by the force of the blast: its skin, when first visited, was as hard as leather, and, like the bodies of all the men, covered with a white mould: it was dragged whole to the shaft, and sent to bank in a net. After the atmospheric air acted a short time upon it, its skin and flesh soon lost their solidity, and became putrid.

August the 1st. The men, who had been working in the two boards north of number 87, made their escape up the wall in which he was found, to the crane-board, and thence down the head-ways. They called on him as they passed his board, but he made no answer. As he had been late up the night before he is supposed to have been asleep when the accident happened. He was not at the place in which he was found when the men alluded to passed it: it therefore appears that he had made a struggle to escape after it was too late to be successful. A day or two before his death he told some of his friends that he had a strong presage upon his mind that he had only a very short time to live: but who has not many times predicted his death before it arrived?

Number 88, discovered on the 3d of August, had the charge of a trap-door in the wall, in which 87 was found. Nature had left something deficient in his brain, which caused an employment to be assigned him in which little memory and contrivance were required. He was found close to the crane, under a very heavy fall.

All the trap-doors and stoppings in this part of the mine were standing when the workmen escaped. The lamp at the crane was still burning. They found no falls in their way out, nor saw any injury done by the first explosion. But when it came to be explored at this time, the stoppings and trap-doors were blown down, the roof fallen, and as great marks of destruction as in any other part of the mine. It is therefore probable that the atmospheric current passing each way, along the double head-ways, intercepted the progress of the first explosion, and prevented its igniting the fire-damp here. But the choak-damp, pressing up the head-ways to occupy the space of the atmospheric air, threw a train of fire-damp from hence into some part of the mine where the coal was burning, and this little magazine was blown up. Perhaps this may serve to explain the cause of the second explosion.

The workmen now began to be employed in carrying on a regular ventilation through the wastes of the mine by stoppings of brick.

On Thursday, the 6th of August, they found that the stable board had been on fire, and that the solid coal was reduced to a cinder, two feet in thickness. As far as the fire had extended the roof was more fallen than in any other part of the mine. At this time it was ascertained that this fall occurred on the 14th of July. The fire here had probably been caused by the hay igniting at the explosion, and communicating to the coal. The air, too, while the pits were open, would have its strongest current up this board, and consequently keep the fire alive. This was the only place in which the solid coal had been on fire. In other parts the barrow-way dust was burnt to a cinder, and felt under the feet like frozen snow.

Number 89 was found under six or seven feet of stone. From this time the ventilation, and search for the remaining bodies, were uniformly persevered in, till September the 1st, when number 90 was discovered; he had been narrowly missed by some persons who visited this part in the dark, on the 18th of July.

The ventilation concluded on Saturday the 19th of September, when number 91 was dug from under a heap of stones. At six o'clock in the morning the pit was visited by candle-light, which had not been used in it for the space of 117 days; and at eleven o'clock in the morning the tube-furnace was lighted. From this time the colliery has been regularly at work; but the body of number 92 has never yet been found.

All these persons (except numbers 1, 4, 5, and 50, who were buried in single graves) were interred in Heworth Chapel-yard, in a trench, side by side, two coffin deep, with a partition of brick and lime between every four coffins. Those entered as *unknown* in the burial register have had names added to them since the search was discontinued.

Article V.

Account of a Chalybeate Spring in the Isle of Wight. By Dr. Waterworth.

(To Dr. Thomson.)

SIR,

Any discovery that promises to be an advantage to mankind, more especially if it tends to improve the art of healing, and thereby lessens the calamities incident to the human body, cannot, I apprehend, but be interesting to the public in general, and to medical men in particular. With this view, therefore, I beg leave to submit to the attention of your readers a short account of a mineral water, which about five years since I accidentally discovered on the southern coast of this island. This water, on examination, not only by the taste but by the application of chemical re-agents, was found to contain sulphate of iron and sulphate of alumina; substances which though rarely met with in combination with water, yet exist in this in such large proportions as to give it a very distinguishing character, and render the other ingredients which enter into its composition wholly imperceptible to the palate. As I have not been able to learn that any mineral water of the same class has hitherto been discovered in Europe, possessing such powerful properties as the sand rock spring, I shall take the liberty of transcribing in his own words the result of the several experiments which that very accurate chemist Dr. Marcet has made on this water, in order to determine its component parts, and which he has made the subject of a very valuable paper published in the first volume of the Transactions of the Geological Society of London. It appears from Dr. Marcet's analysis that each pint, or 16 ounce measure, of aluminous chalybeate water, contains the following ingredients:—

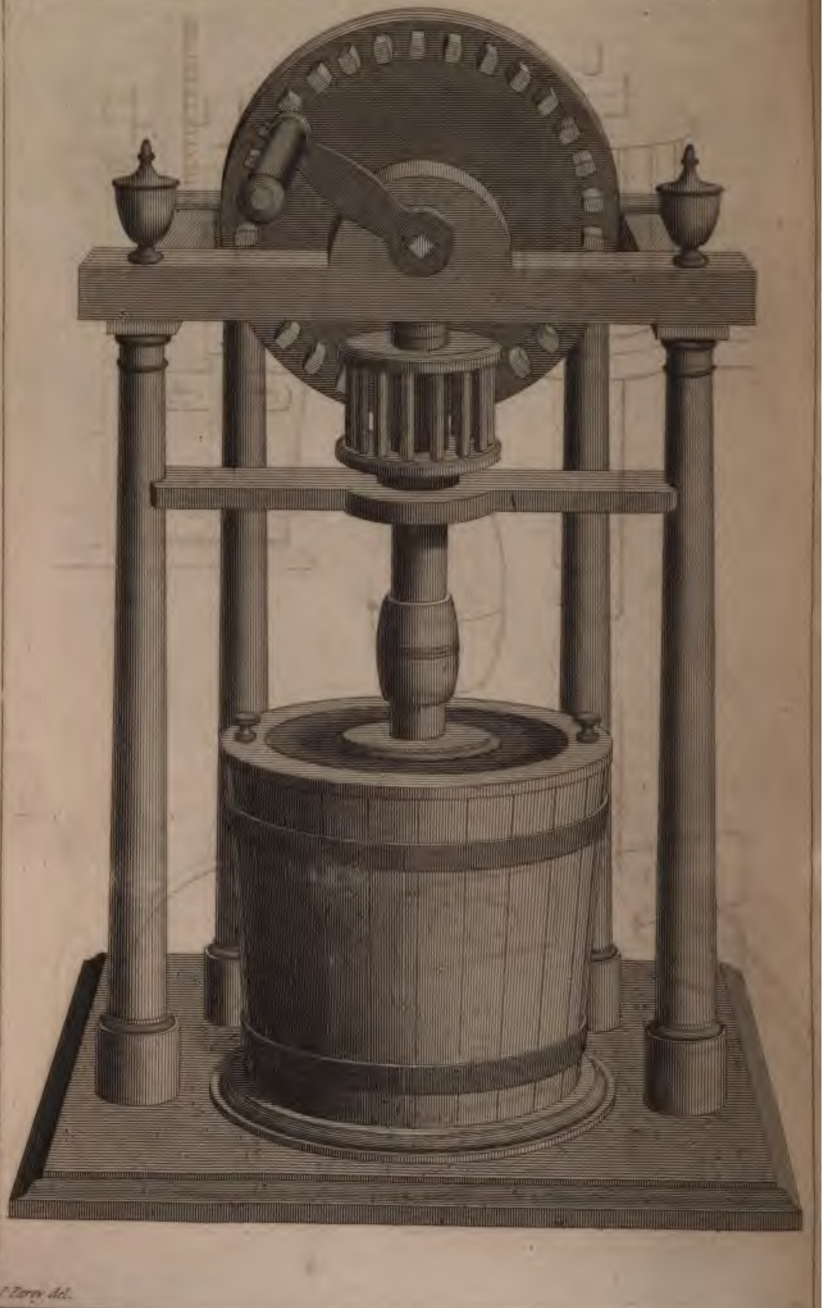
	Grains.
Of Carbonic acid gas three-tenths of a cubic inch.	
Sulphate of iron in the state of crystallized green sulphate	41·4
Sulphate of alumina, a quantity of which, if brought to the state of crystallized alum, would amount to	31·6
Sulphate of lime, dried at 160°	10·1
Sulphate of magnesia, or Epsom salt crystallized	3·6
Sulphate of soda, or Glauber's salt crystallized	16·0
Muriate of soda, or common salt crystallized	4·0
Silica	0·7
	107·4

Dr. Marcet goes on further to state, that he is not acquainted with any chalybeate or aluminous spring in the chemical history of mineral waters that can be compared, in regard to strength, with that just described. The Hartfell water in Scotland, and the Harley Green Spaw near Halifax in Yorkshire, both of which appear to be analogous to this in chemical composition, and were considered as the strongest impregnations of the kind, are stated by Dr. Garnett to contain, the one only about 14 grains, and the other 40 grains, of saline matter in each pint.

The tonic powers of the sulphate of iron, as applicable to a great variety of painful and obstinate diseases, even when administered under the ordinary forms of prescription, and more particularly when given in a state of dilution, are well known to the profession; and the advantages to be derived from a further combination of this active remedy with alum so prepared by nature, as to admit of its being applied in very considerable proportions over a large surface of the stomach without injuring that organ, are circumstances which I am convinced will not escape the observation of medical men. Since the period of my first discovering this water, and ascertaining its properties, I have employed it very extensively both in my public and private practice; and the result of my experience of its effects has proved it to be a tonic of the most powerful kind, and, as such, singularly efficacious in the cure of all diseases termed asthenic, arising from a relaxed habit and languid circulation.

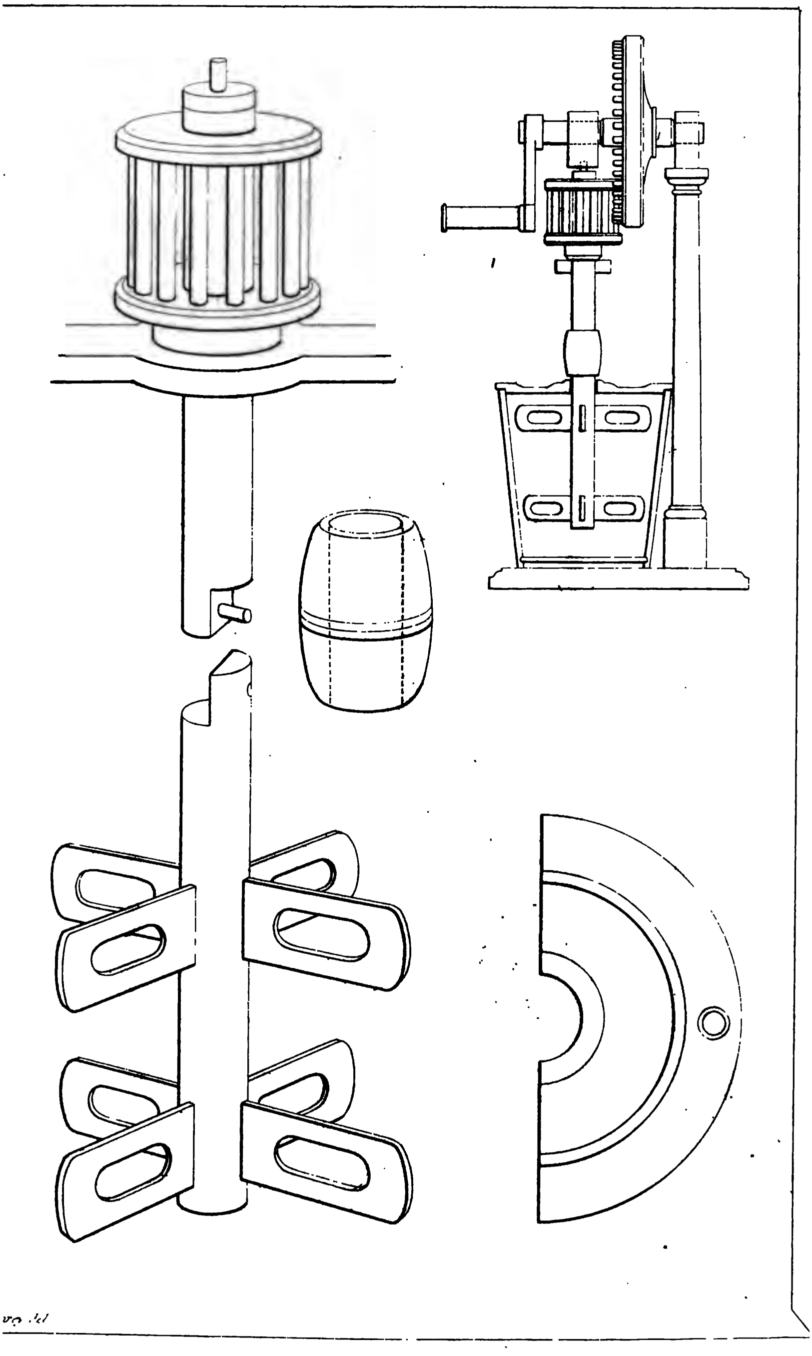
In addition to the high opinion which I have been enabled to form of its good effects, I am happy to have it in my power to add the testimonies of other medical men of the first respectability, particularly of my friends Dr. Saunders and Dr. Lempriere, the former of whom, in a letter I some time since had the honour to receive from him, informed me he had experienced the beneficial effects of the water in cases of uterine hæmorrhagy, excessive discharges of fluor albus, and in incipient cases of diseased uterus, so as to prevent the progress to ulceration, as also in chlorosis; and that he was persuaded it would be found useful in dyspeptic cases, and in chronic diarrhœas. The latter Gentleman, who is physician to the forces at the army depôt hospital in the island, has authorised me to say he has given the water in upwards of 200 cases at that establishment (where he also still continues to administer it) principally consisting of those terminating in, or connected with, chronic debility; but more particularly in patients who have been reduced by long residence in warm climates, by visceral obstructions, obstinate intermittents, chronic rheumatism, and the like; and where the ordinary tonics, both of the vegetable and mineral kingdom, had failed to produce the desired effects. In such cases, provided the thorax and abdominal viscera had not been materially impaired; or, if they had been previously diseased, the more important symptoms had been removed; this mineral water never failed to produce the most speedy and most beneficial effects, such as he had not before observed in any other remedy, evinced by a rapid improvement in the patient's countenance, spirits, and appetite, and ultimately by a permanent establishment of his health.

The mode in which I have usually administered this medicine, and which I believe in general has been adopted by others, has



J. Farey del.

SPANISH CHURN.



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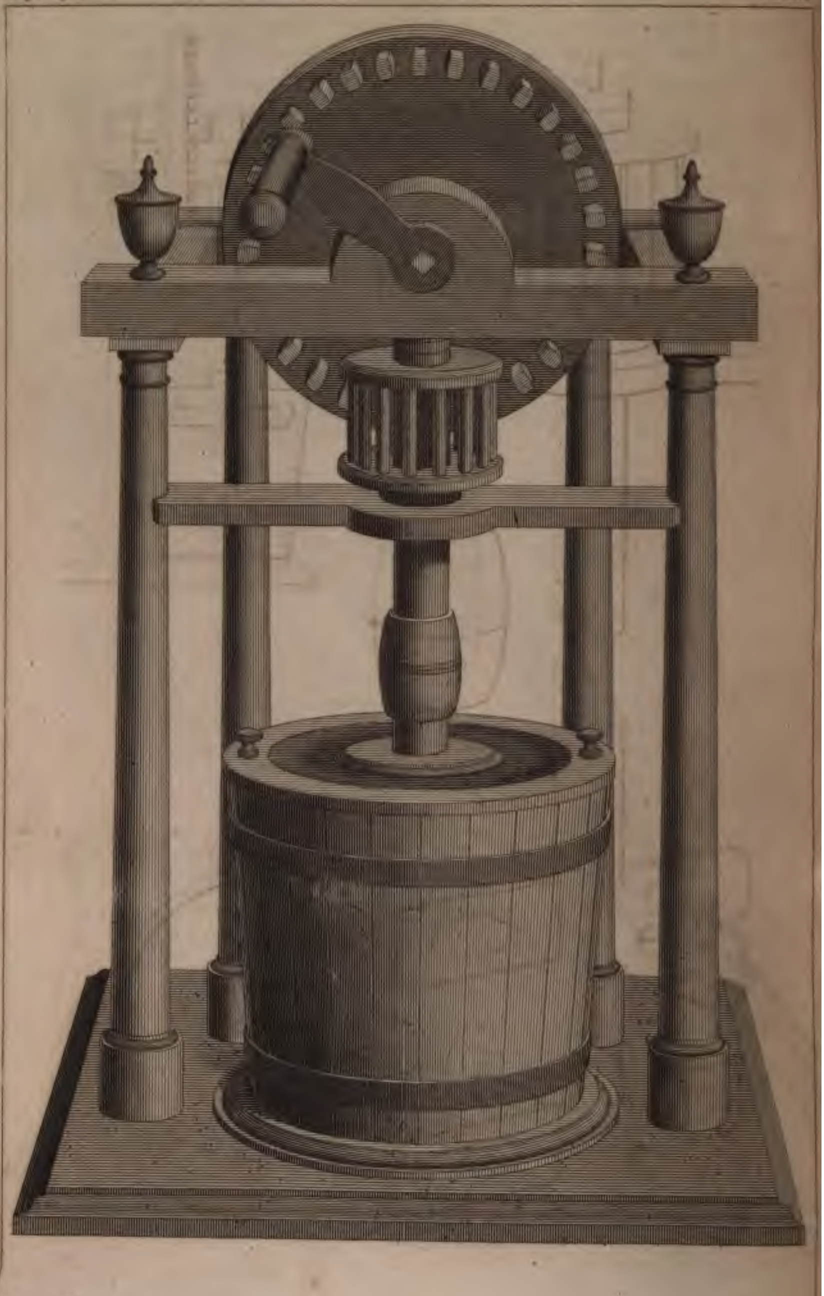

J. Farey del.

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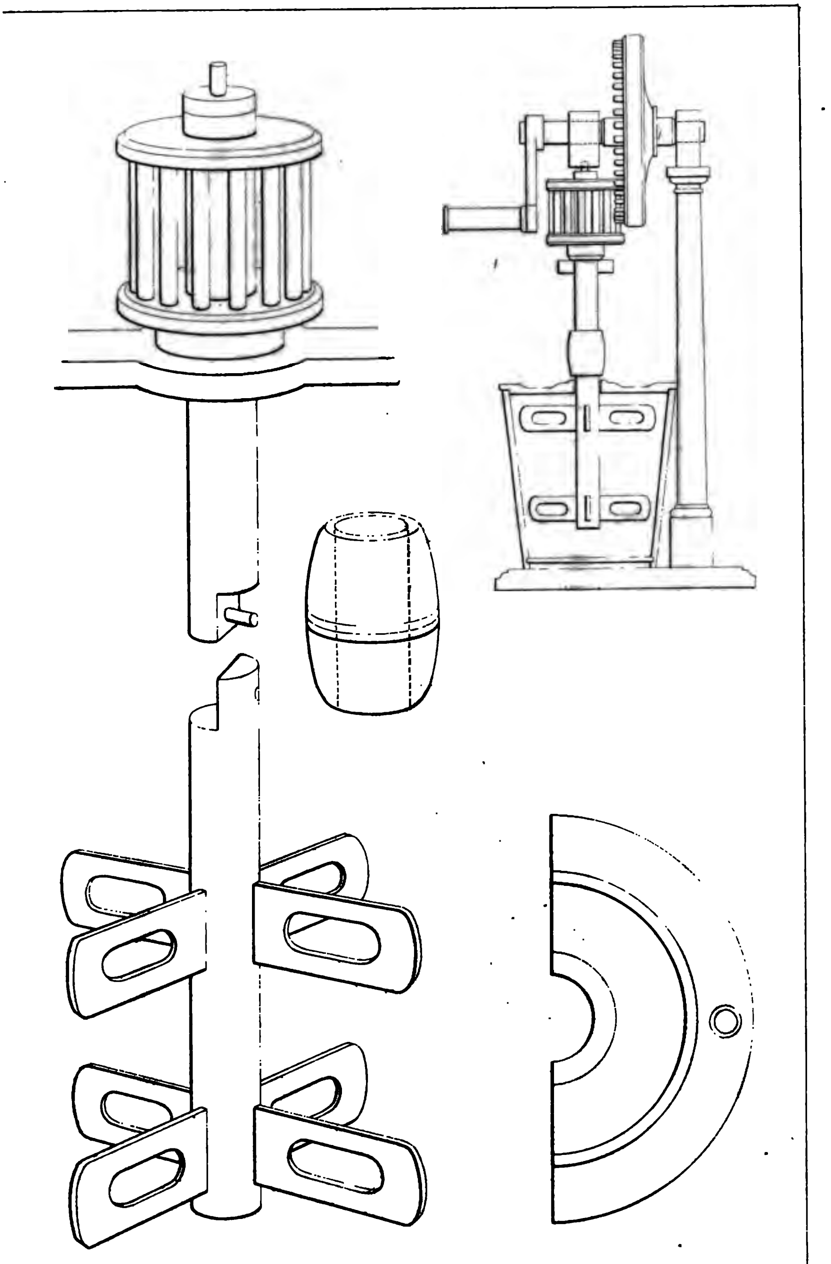


SPANISH CHURN.

Article VI.

Description of a Churn. By H. Robertson, M.D.

(With a Plate.)

The model from which the drawing (plates 6 and 7) was taken, resembles considerably the frame of an air-pump. Its base is a board of 12½ inches by 9; from which ascend four pillars in two rows, 17½ inches in height; the front row is 9 inches distant. Those forming the back row are placed at an interval of 7½ inches. These are joined together at the top by cross pieces of wood, and are 5 inches separated. Upon the top of the pillars there is a perpendicular wheel 7 inches in diameter, with a row of pegs round the margin to the front; it turns by a winch handle, projecting from the machine, and throws the wheel by an axis running through a semicircular piece of wood fixed upon the cross beams: 3½ inches below the beam which joins the tops of the pillars of the front row there is another cross beam, stretching between the pillars, and which is perforated to admit a cylindrical piece of wood: and between the upper and lower beams there is an horizontal wheel, with perpendicular spokes ⅓ inch distant from each other. This wheel resembles the capstan of a ship. The cylindrical piece of wood which penetrates the lower beam passes through this wheel, to which it is fixed, the top of it moving easily in a groove of the upper beam. The cylindrical piece of wood descends below the lower beam about 6½ inches, the extremity of which has a groove and a notch, that it may join with a piece of wood of a corresponding form, about 10 inches in length, the lower part of which is armed with four pieces of wood 2 inches long, by 1½ in breadth, ⅓ inch thick, and perforated with holes. The joining of the cylinder is secured by a wooden ring, made with a groove to adapt it to the lower end of the notch. The lower part of the cylinder descends into a barrel 7½ inches deep, and in diameter 7 inches. The barrel is covered by two pieces of wood, which fit exactly to its brim, with a space to admit the play of the descending cylinder; and that the milk in the operation of churning should not be dashed out. The barrel is made of hard wood, hooped with brass, and is kept firm in its place by a raising upon the board forming the base.

This kind of churn is used in Spain. The Editor has been informed that there is one of a similar construction driven by water, that has been for some time used in Aberdeenshire.

Article VII.

Tables of Weights and Measures.

The diversity in the weights and measures of different nations occasions considerable trouble to the readers of foreign books of science, and sometimes even prevents them from fully comprehending their authors. On that account we think it will be attended with considerable convenience to our readers if we insert in this place tables of the most common weights and measures that occur in foreign books. As this journal may likewise fall into the hands of foreigners, we shall for their convenience insert also tables of the weights and measures used in England.

I. *English Weights and Measures.*

There are two kinds of weights used in England; namely, Troy weight and Avoirdupois weight. The smallest weight in both is the *grain*, which was conceived when first brought into use, to be equivalent to the weight of a grain of ripe barley well dried. The Avoirdupois weight is the one in common use, and is employed to weigh all heavy articles. Hence it does not usually descend so low as the grain. Troy weight (so called, according to Dr. Smith, because employed at a great annual fair at Troyes, a town in France) is used for weighing gold and silver, apothecaries' goods, and some other articles. It is the weight always employed in English chemical books, except when the contrary is expressly stated.

1. Troy weight.

24 grains make	1 pennyweight, marked dwt.
20 pennyweights	1 ounce, marked oz.
12 ounces	1 pound, marked lb.

2. Avoirdupois weight.

16 drams make	1 ounce.
16 ounces	1 pound.
28 pounds	1 quarter, marked qrs.
4 quarters	1 hundred weight, marked C.wt.
20 hundred weight	1 ton, marked T.

3. Troy weight, by Apothecaries, is divided as follows, and is then known by the name of Apothecaries' weight.

20 grains make	1 scruple, marked ℈.
3 scruples	1 dram, marked ʒ.
8 drams	1 ounce, marked ℥.
12 ounces	1 pound.

An Avoirdupois pound is equal to 7002 Troy grains. From this all the parts of Avoirdupois weight may be easily estimated.

4. English measures of length.

3	inches make	1 palm.
2·64	palms	1 Gunter's link.
1·136	Gunter's links or 3 palms	1 span.
$1\frac{1}{3}$	span or 12 inches	1 foot.
$1\frac{1}{2}$	feet	1 cubit.
3	feet or two cubits	1 yard.
$1\frac{2}{3}$	yards or 5 feet	1 pace.
$1\frac{1}{5}$	pace or six feet	1 fathom.
$5\frac{1}{2}$	yards	1 pole.
4	poles or 66 feet	1 Gunter's chain.
40	poles	1 furlong.
8	furlongs or 1760 yards	1 mile.

The English ell contains one yard and a quarter, or 45 inches.

5. English wine measure.

28·875	cubic inches make	1 pint.
2	pints	1 quart.
4	quarts	1 gallon.
18	gallons	1 rundlet.
$31\frac{1}{2}$	gallons	1 barrel.
42	gallons	1 tierce.
63	gallons	1 hogshead.
84	gallons, or $1\frac{1}{3}$ hogshead	1 puncheon.
$1\frac{1}{2}$	puncheon	1 but.
2	buts or 4 hogsheads	1 ton.

Sir George Shuckburgh Evelyn determined, by very accurate experiments, that the difference between the length of two pendulums vibrating 42 and 84 times in a minute of mean time, in the latitude of London, at 113 feet above the level of the sea, in the temperature of 60° Fahrenheit, and when the barometer stands at 30 inches, is 59·89358 inches of parliamentary standard. A cubic inch of distilled water, when the barometer stands at 29·5 inches, and the thermometer at 60° weighs according to a mean of Sir George Shuckburgh's experiments 252·506 grains Troy, and in vacuo 252·806 grains.* From these data the English weights and measures might be restored again supposing them lost.

II. *French Weights and Measures.*

There are two sorts of French weights and measures that require to be known; namely, the old ones employed before the

* Phil. Trans. 1798, vol. lxxxviii. p. 133.

revolution and the new ones introduced by the revolutionary government.

1. Old French weights.

The Paris weights used by all men of science before the revolution, are divided as follows.

72 grains make	1 gros.
8 gros	1 ounce.
16 ounces	1 pound.

A French denier or scruple is 24 grains: a marc is 8 ounces, or half a pound.

From a paper in the Transactions of the Royal Society, (Phil. Trans. 1742, Vol. xlii. p. 285) we learn that the Paris pound weighs exactly 7560 Troy grains. Hence the French

Grain is equal to	0·8203125 Troy grains.
The gros	59 0625
The ounce	472·5

To reduce old French weights into English grains, we have only to multiply them by these numbers respectively.

2. Old French measures of length.

12 lines make	1 inch.
12 inches	1 foot.
6 feet	1 toise.

It appears from an exact measurement made by Dr. Maskelyne, that at the temperature of 61° the Paris toise is equal to 76·7344 English inches.* Hence we have

The French line = 0·088813 English inches.
inch = 1·065755
foot = 12·789060

To reduce old French measures to English inches, we have only to multiply by these numbers respectively.

3. Old French measures of capacity.

4 poisons make	1 chopine.
2 chopines	1 pinte.
8 pintes	1 septier or velte.
36 septiers	1 muid de vin.

The pinte was legally equal to 48 cubic French inches. From this all the other measures of capacity may be easily deduced. A French cubic foot is equal to 1·2105 English cubic feet. Hence French cubic feet may be converted into English by multiplying by that number.

* Phil. Trans. 1768, vol. lviii. p. 274.

III. *New French Weights and Measures.*

After the revolution the French introduced a new set of weights and measures, all decimal; the basis from which they were all constructed was the *metre*, which was made equal to the ten millionth part of the quadrant of the meridian. The metre was carefully compared with an English standard measure of 49 inches in length, made by Troughton, and lent by Mr. Pictet to a committee of the French Institute for the purpose. From this comparison, attending to some observations on the subject made by Dr. Young, the following tables of the values of the new French measures and weights in English measures and weights have been constructed.

1. Measures of length, the metre being at 32°, the foot at 62°.

	English Inches.
Millimetre	0·03937
Centimetre	0·39371
Decimetre	3·93710
Metre	39·37100
Decametre	393·71000
Hecatometre	3937·10000
Chiliometre	39371·00000
Myriometre	393710·00000

	Miles.	Fur.	Yds.	Ft.	Inches.
A decametre is	0	0	10	2	9·7
A hecatometre	0	0	109	1	1
A chiliometre	0	4	213	1	10·2
A myriometre	6	1	156	0	6

8 chiliometres are nearly 5 miles.

2. Measures of capacity.

	Cubic Inches English.
Millilitre or centimetre cube	0·06103
Centilitre	0·61028
Decilitre	6·10280
Litre, or cubic decimetre	61·02800
Decalitre	610·28000
Hecatolitre	6102·80000
Chiliolitre	61028·00000
Myriolitre	610280·00000

A litre is nearly 2$\frac{1}{8}$ wine pints: 14 decalitres are nearly 3 wine pints. A chiliolitre is 1 tun, 12·75 wine gallons.

3. Weights.

A *gramme* is the weight of a cubic centimetre of pure water at its maximum of density. It has been found equal to 18·827 French grains, of which 576 made 472·5 English; and 489·5058 grammes make a pound of the standard of the mint at Paris.

	English grains.
Milligramme	0·0154
Centigramme	0·1544
Decigramme	1·5444
Gramme	15·4440
Decagramme	154·4402
Hecatogramme	1544·4023
Chiliogramme	15444·0234
Myriogramme	154440·2344

A decagramme is 6 dwts. 10·44 grains Troy; or 5·65 drams avoirdupois. A hecatogramme is 30 ounces, 8·5 drams avoirdupois. A chiliogramme is 2 lbs. 3 oz. 5 dr. avoirdupois. A myriogramme is 22 lbs. 1·15 oz. avoirdupois. 100 myriogrammes are 1 ton, wanting 32·8 lbs.

4. Agrarian measures.

Are, 1 square decametre .... 3·95 perches.
Hectare........... 2 acres, 1 rood, 35·4 perches.

5. For firewood.

Decistere, $\frac{1}{10}$ stere ...... 3·5317 cubic feet English.
Stere, 1 cubic metre .... 35·3171 cubic feet.

6. Money. Copper.

Centime, 1 gramme	15·4	English grains.
5 centimes, or sous	77·2	
Decime	154·4	
2 Decimes	308·8	

Silver $\frac{9}{10}$ or $\frac{36}{40}$ fine.

Franc, 5 grammes .... 3 dwts. 5·2 grains.
5 francs .... 16 dwts. 2·1

The franc is nearly the same as the livre tournois, and worth about 10d.

IV. *German Weights and Measures.*

The weights used in Germany are very various. But those that usually occur in chemical books are the apothecaries' weights, commonly known by the name of the Nuremburg

Medicinal weights. They are divided precisely as our English Apothecaries' weight, or as follows:—

20 grains make	1 scruple.
3 scruples	1 dram.
8 drams	1 ounce.
12 ounces	1 pound.

According to Gren, 1 Nuremburg lb. = 0·959266 of an English Troy pound. The same ratio exists between the respective ounces, drams, scruples, and grains. Hence, to reduce Nuremburg weights to their respective English, we have only to multiply by 0·959266.

There is another weight in common use in Germany, namely, the Cologne mark, or mark of Charlemagne. It is employed in weighing gold and silver, and therefore often occurs in chemical books. It is thus subdivided:—

256 standard parts (Recht pfenningtheil) make	1 pfenning.
4 pfennings (pennyweights or deniers)	1 quentchen.
4 quentchens (drams)	1 loth.
2 loths	1 ounce.
8 ounces	1 mark.

It appears from Gren that 1 Cologne ounce is equal to 0·939018 of a Troy ounce, and the Cologne marc is equal to 7·512144 Troy ounces. From these data it is easy to reduce the Cologne weights to the English standard.

The common measure of length in Germany is the Rhinland foot, which, like our own, is divided into 12 inches.

	English Inches.
1 Rhinland foot =	12·341
1 Rhinland inch =	1·02842

V. *Swedish Weights and Measures.*

The Swedish pound is divided in the same manner as our apothecaries' pound. A Swedish pound weighs 6556 grains Troy. Hence an English Troy grain is equal to 1·138194 Swedish grains. And to reduce Swedish grains to English we must multiply by that number.

The Swedish inch is equal to 1·238435 English inches.

The Swedish kanne is equal to 100 Swedish cubical inches. It is equal to 189·9413 English cubic inches.

The lod, a weight sometimes used by Bergman, is the 32d part of a Swedish pound.

Article VIII.

Astronomical and Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude 51° 32′ 40″ North. Longitude West in Time 6·82″.

1813.

				Time at H. W.	Time at Greenwich.
April 22,	Emersion of Jupiter's	4th satellite		8h 56′ 59″	8h 57′ 05·82″
April 26,	Immersion	Ditto,	2d Ditto	8 33 01	8 33 07·82
May 4,	Emersion	Ditto,	1st Ditto	9 16 15	9 16 21·82
May 15,	Emersion	Ditto,	2d Ditto	10 04 19	10 04 25·82

N. B. The Observations are according to Mean Time.

Rain between the 31st of March and the 1st of May, 1,624 inches.

In the observations set down in the *Annals of Philosophy*, N° IV. an error is committed in deducing the time at Greenwich; the longitude should have been added to the time at H. W., instead of being subtracted.

Observations on the Variation of the Needle continued.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
April 17	9h 00′	24° 08′ 15″	1h 20′	24° 22′ 05″	6h 30′	24° 15′ 52″
Ditto 18	8 45	24 04 15	1 30	24 24 52	8 30	24 11 40
Ditto 25	— —	— — —	1 15	24 24 12	— —	— — —
Ditto 26	— —	— — —	— —	— — —	7 00	24 19 40
Ditto 27	— —	— — —	— —	— — —	7 00	24 17 42
Ditto 28	— —	— — —	1 15	24 22 07	— —	— — —
Ditto 29	8 25	24 14 55	— —	— — —	5 30	24 15 20
Ditto 30	— —	— — —	1 10	24 20 40	7 20	24 12 35
Mean	8 31	24 09 18.	12 59	24 21 12	5 46	24 15 25

The mean of all the observations made in April, and including those of the 30th and 31st of March, is as follows:—

Observations	Morning at 8h 31′	..... Variation	24° 09′ 18″	West.
	Noon at 12 59	..... Ditto	24 21 12	
	Evening at 5 46	..... Ditto	24 15 25	

Month.	Morning Observ. Hour.	Morning Observ. Variation.	Noon Observ. Hour.	Noon Observ. Variation.	Evening Observ. Hour.	Evening Observ. Variation.
May 1	— —′	—° —′ —″	1h 25′	24° 23′ 39″	6h 30′	24° 08′ 50″
Ditto 2	8 05	24 08 21	1 25	24 22 24	6 07	24 12 11
Ditto 3	8 50	24 09 45	2 35	24 26 20	6 35	24 12 05
Ditto 4	9 12	24 12 25	1 35	24 16 30	6 00	24 12 16
Ditto 5	8 50	24 10 32	1 20	24 21 59	7 35	24 13 45
Ditto 6	— —	— — —	— —	— — —	6 45	24 13 38
Ditto 7	9 12	24 13 42	1 47	24 17 42	— —	— — —
Ditto 8	9 05	24 11 15	1 27	24 19 00	6 45	24 11 42
Ditto 9	8 40	24 11 47	1 28	24 19 43	7 40	24 14 00
Ditto 10	8 10	24 11 39	— —	— — —	6 10	24 14 42
Ditto 11	8 30	24 14 27	1 35	24 21 34	5 07	24 13 52
Ditto 12	8 55	24 11 01	1 20	24 19 40	5 07	24 13 51
Ditto 15	— —	— — —	1 15	24 22 07	6 20	24 12 50
Ditto 16	7 50	24 09 32	1 50	24 21 05	— —	— — —
Ditto 17	8 12	24 10 32	1 30	24 20 12	6 22	24 14 02

It is remarkable that on the 3d of May the maximum of the variation was not until 35 minutes past 2; at that time there was a great deal of thunder in the west: but on the next day, May the 4th, in the afternoon, and when a thunder storm was in the east, the variation was only 24° 16′ 30″, the difference being 9′ 30″.

Errata in the last observations of N° V. p. 371.

March 31,	*for* 24° 11′ 42″	*read* 24° 21′ 42″
April 9,	*for* 11h 35′	*read* 12h 35′
Ditto 11,	*for* 11 25	*read* 12 25
Ditto 12,	*for* 11 30	*read* 12 30
Ditto 13,	*for* 11 45	*read* 12 45

Article IX.

Analyses of Books.

I. *Memoirs of the Literary and Philosophical Society of Manchester. Second series.* Vol. II. 1813, pp. 484.

This volume contains the following papers:—

1. *An Account of some Experiments to ascertain whether the Force of Steam be in proportion to the generating Heat.* By John Sharpe, Esq.] It is well known that the elasticity of steam increases at a great rate with the temperature. At 212° its elasticity is 150 times greater than at 32°, and at 252° its elasti-

city is twice as great, and at 307° four times as great as 212°. Now as steam is very much employed as a moving force, it becomes a question of considerable importance to determine the cause of this increased elasticity. From the experiments of Mr. Watt we learn that the latent heat of steam is 940°, while Count Rumford makes it 1020°. Now is the increased elasticity of steam owing to an increase in the latent heat of steam, or to an increase of its density? Mr. Sharpe's experiments seem conclusive in favour of the last of these opinions. He ascertained two things: 1. That water heats equably, or in the same time (supposing the heating cause the same) from 120° up to the highest temperature that it can reach without boiling (and that temperature depends upon the pressure). Suppose, for example, that it is heated 10°, or from 120° to 130°, in three minutes; it will be heated from 270° to 280° in the same time. This is a very curious fact, and not easily explained, unless we have recourse to Mr. Dalton's supposition, that the thermometer is an inaccurate measurer of heat. 2. That six ounces of steam of 212° condensed into water give out as much heat as six ounces of steam at the temperature 275°; but the second six ounces come over in a much shorter period than the first. This I conceive to be a demonstration that the increased elasticity of steam is owing to a corresponding increase of its density. Therefore the density of steam at 212° is 150 times greater than at 32°; and its density at 252° is twice as great as at 212°. Hence we have the specific gravity of steam at different temperatures, as follows:—

	Sp. Gravity.
At 32°	0·0046
212	0·6896
252	1·3792
307	2·7584

This explains the elasticity of steam in a satisfactory manner, and brings it under the same law as common air, and all the other elastic fluids.

2. *On Respiration and Animal Heat.* By John Dalton.] This paper was read to the Manchester Society in 1806. There is an appendix to it, written in 1810; and a second appendix, written in 1811. It exhibits marks of all the originality and ingenuity so characteristic of Mr. Dalton; and had it been published when originally written, it would have produced a great improvement in the opinions of physiologists relative to respiration. But the opinions contained in this essay are nearly the same as those generally embraced by physiologists; partly in consequence of Mr. Dalton's own opinions, which have been long known to the world, and partly on account of the accurate

and valuable experiments of Allan and Pepys on the subject. The phenomena of respiration as described by Dalton in this paper, and as generally admitted, are as follows:—A portion of the oxygen of the air inspired disappears, and is replaced by an equal bulk of carbonic acid gas. The air expired is saturated with moisture, and its temperature is raised to about 98°. Mr. Dalton adopts the theory of Crawford, that respiration is the source of animal heat; and in his second appendix he endeavours to refute the objections to this theory brought by Mr. Brodie. Crawford's theory depends upon the two following propositions: 1. The specific heat of oxygen gas is greater than that of carbonic acid. 2. The specific heat of arterial blood is greater than that of venous blood. If these propositions be inaccurate, Crawford's theory falls to the ground. Now in the last number of the *Annals of Philosophy* (p. 389) there is a table of the specific heat of the gases from the experiments of Delaroche and Berard, from which it appears that the specific heat of oxygen gas is to that of carbonic acid gas as the numbers 0·2361 : 0·2210. Now if these numbers be correct, the first of Crawford's propositions falls to the ground. Suppose 20 cubic inches of air to be drawn into the lungs by one inspiration, and that one cubic inch of oxygen gas is converted into carbonic acid gas; from the above numbers we see that $\frac{1}{15}$th of the heat of the oxygen gas would be disengaged. Let us suppose the temperature of the air inspired to be 60°, and that of the air expired to be 98°. The whole 20 cubic inches of the air will have been heated 38°. Now let us suppose the absolute quantity of heat in the oxygen to be 10000°; the quantity of heat thus disengaged would be 666°. This being divided into 20 portions would heat the whole air only 33°, or from 60° to 93°; so that the whole heat evolved would not be sufficient to raise the air inspired to the temperature of the body. In fact, it would not produce so great an effect as I have supposed; for, according to the table of Delaroche and Berard, the specific heat of azote is 0·2754; so that a degree of heat from oxygen gas would not heat azotic gas quite a degree. It follows from this, that if the table of Delaroche and Berard be correct, Crawford's theory of animal heat must be given up. I do not know how the experiments of the French Gentlemen were made, and therefore cannot say what confidence may be placed in them; but I have seen a letter by Berthollet, in which he declares that he has the most perfect confidence in their accuracy. Now Mr. Dalton will acknowledge, I think, that Berthollet is not very apt to adopt the opinions of others without pretty strong evidence. I think, therefore, we are warranted at present in rejecting Crawford's theory altogether, till new and indisputable evidence be produced of the truth of his fundamental principles, which are entirely overturned provided the French experiments be accurate.

3. *An Inquiry into the Principles by which the Importance of foreign Commerce ought to be estimated.* By Henry Dewar, M. D.] This paper appears to have been written in consequence of the attempts of Bonaparte to destroy the foreign commerce of Great Britain, and the probability that these attempts would be crowned with success: in consequence of the well-known pamphlet written by Mr. Spence entitled, *Britain independent of Commerce,* and the various answers made to that pamphlet. Dr. Dewar considers the effect of foreign commerce upon the wealth, the population, the happiness, and the power, of this country. There can be no doubt, he thinks, that it increases the wealth of the country. Its effects in promoting the population are, in his opinion, confined to the additional food which it imports into the country. He seems inclined to think that foreign commerce at present does not increase the happiness of the country: though he conceives that it might, perhaps, be so regulated as even to add to the sum of national happiness. He considers Mr. Spence as having demonstrated that the power of the country is independent of foreign commerce; that the loss of it would occasion considerable sacrifices; but that they might be borne without ruin: and that even supposing foreign commerce destroyed, we might still retain the sovereignty of the sea, and keep up our land forces as we do at present.

4. *Remarks on the Use and Origin of Figurative Language.* By the Rev. William Johns.] The author conceives that words were chiefly used at first in a figurative sense from necessity; because the language did not afford any other means of expressing the idea which it was the object of the speaker to convey. In process of time many of these words lost their original signification, and were only used in their figurative sense: thus they ceased to be figurative. Mr. Johns thinks there can be no doubt that language at first consisted of nothing but *nouns*; and that all other words, adjectives and verbs for example, were only nouns used in a figurative sense: though in process of time many of these words lost their original meaning, and came to be used only as adjectives or verbs.

5. *On the Measure of Moving Force.* By Mr. Peter Ewart.] This is a very long and most able defence of a doctrine which has been in some measure proscribed both in this country and in France. The subject occasioned a most violent controversy among mathematicians, which, after continuing for above 30 years, was at last dropped about 70 years ago; and the general opinion at present entertained upon the subject is, that it was nothing else than a dispute about terms. The question was, whether mechanical force is to be measured by the mass multi-

plied into the velocity, or into the square of the velocity. The last of these opinions was adopted by Hooke and by Huygens in consequence of their observations on the motions of pendulums. It was also adopted by Smeaton in consequence of his experiments on the mechanical action of water. It was embraced by Leibnitz, and by his particular friends and supporters, the Bernoullis. And as the opposite opinion had been advanced by Newton, it became involved in the Leibnitzian controversy with the Newtonians. All the followers of Leibnitz embraced his determination, while the Newtonians as strenuously adopted the opinion of Newton. The French were induced to coincide with the Newtonians, because the same measure of force had been employed by Descartes. Thus the measure of force adopted came to be the distinguishing feature of two opposite and hostile sects, and each party rather struggled for victory than for the cause of truth. That this party spirit is not quite extinguished in this country I had a very remarkable proof soon after the publication of Dr. Wollaston's lecture on the subject in 1806. I was told by a very sensible Gentleman, a good mathematician, and one who had attended particularly to natural philosophy, that Dr. Wollaston must be ignorant of the first principles of mechanics, because he defended the opinions of Mr. Smeaton. Such is the zeal that still remains in this country in favour of the Newtonian doctrine. Mr. Ewart supports the opinion of Smeaton with great force of reasoning. The essay is remarkable for the extensive knowledge of the subject which the author displays, and for the great perspicuity of his reasoning, which is the consequence of this extensive knowledge. He gives a number of examples, which he considers as inconsistent with the common notion, discusses these examples, and gives us a very full history of the opinions of mechanical writers on the subject. I must acknowledge that some of his cases appear to me fully capable of solution according to the common notion; while others are so complicated that it is difficult to disentangle them. The most striking cases in favour of the Leibnitzian doctrine are those in which, if we adopt the common opinion, a rotatory motion seems to be produced without any force at all. We recommend Mr. Ewart's essay to the attention of mechanical philosophers, as by far the best defence of the Leibnitzian doctrine that we have ever seen, and as a striking proof of the sagacity and extensive knowledge of the author.

6. *Account of a remarkable Effect produced by a Stroke of Lightning; in a Letter addressed to* Thomas Henry, Esq. F.R.S. &c. *President of the Literary and Philosophical Society, from* Matthew Nicholson, Esq. *With remarks on the same by* Mr. Henry.] This contains an account of a very uncommon accident, which happened at Mr. Chadwicke's house, about five

miles from Manchester, on the 4th of September, 1809. A very loud explosion of thunder took place, and the front wall of the coal vault, containing about 7000 bricks, and weighing about 26 tons, was gradually lifted up entire, and moved nine feet forwards from its former position. Mr. Henry compares this to the thunder-storm at Coldstream, described by Mr. Brydone in the Philosophical Transactions for 1787, and explained in a very satisfactory manner by Lord Stanhope. He conceives it to have been a case of the *returning stroke*. The lightning issued out of the earth by the coal vault to restore the equilibrium in the clouds over head.

7. *Theorems and Problems intended to elucidate the mechanical Principle called Vis Viva.* By Mr. John Gough.] By *vis viva* Mr. Gough means the whole force opposed by a body in motion to a retarding force which impedes its progress; and conversely, it is the whole force accumulated in a body by the action of any motive force, which puts that body in motion. These theorems and problems being mathematical demonstrations could not be understood in an abridged form. I must therefore of necessity refer the reader to the volume itself.

8. *On the Theories of the Excitement of Galvanic Electricity.* By William Henry, M.D. F.R.S. &c.] Two different explanations of the energy of the galvanic battery have been given: the first entirely electrical, the second chemical. There is a third, in which the two preceding agents are combined together. 1. When zinc and copper are brought in contact, the zinc becomes positive, and the copper negative. Hence there is an accumulation of electricity in the zinc. The imperfect conductor in contact with the zinc receives this redundancy, and conveys it to the second plate of copper. In this manner the electricity travels from one end of the battery to the other, and accumulates as it proceeds in a geometrical ratio. Such was Volta's original explanation. Mr. Cuthberton accounted for the progressive motion by the chemical action of the liquid on the zinc. But this explanation does not account for the prodigious effects produced by some liquids when compared to others, nor does it show us why all effect ceases with the chemical action of the liquid on the plates; nor why hydrogen gas is evolved from the copper, while it is the zinc that is chiefly dissolved. 2. When a metal is oxydated it gives out electricity. Electricity in a pure state can pass through perfect conductors; but it only passes through imperfect ones when combined with hydrogen. Hence the evolution of electricity depends upon the chemical action of the metal on the liquid. Hence the hydrogen is evolved from the copper. Hence the energy of the battery is proportional to the number of pairs of plates. Such is the chemical theory of Dr. Bostock; but it is founded on premises

which have never been proved to exist, and assumes the invisible passage of a current of hydrogen gas through a liquid, which it is difficult to understand. 3. Sir Humphrey Davy has given a theory of the galvanic energy, in which he unites the two preceding explanations. He conceives that when the battery, for example, is composed of copper, zinc, and solution of common salt, the zinc becomes positive, and the copper negative. Therefore the zinc attracts the oxygen and acid, which are negative; and the copper, the hydrogen and alkali, which are positive. But this equilibrium is immediately destroyed by the formation of muriate of zinc, and the evolution of hydrogen gas. Hence the action of the zinc and copper is again repeated, and this goes on as long as the chemical action continues.

Dr. Henry is of opinion that the primary excitement of electricity is owing to the chemical changes; but he conceives it to be essential to the activity of the battery that one set of elements of the fluid should have no affinity for one of the metals. Thus in the preceding example the oxygen and the acid combine with the zinc; but the hydrogen and alkali having no affinity for the copper, deposite a portion of their electricity on it, and thus the accumulation proceeds. He accounts for the evolution of the two constituents of a substance decomposed by the battery at the two poles, though at a distance from each other, by supposing a series of intermediate decompositions to go on. Suppose water to be the substance decomposed; we may conceive a series of particles of water arranged between the two poles. An atom of oxygen gas escapes at the positive pole. The hydrogen previously combined with this atom unites with the oxygen of the next particle of water; and this successive decomposition goes on till it reaches the negative pole, when the atom of hydrogen remaining makes its escape in the form of gas. This is a very ingenious explanation, and, as far as water is concerned, appears satisfactory. But it will not apply to other bodies, as Dr. Henry thinks it will. Suppose a weak solution of common salt: we know that the action of the battery may be continued till every particle of the salt is decomposed, and all the acid accumulated round one pole, and the base round the other. Now what must happen when only a single particle of common salt remains to be decomposed? It is clear that no series of decompositions take place; the acid must move to one pole, and the base to the other, through the water.

(To be continued.)

II. *Tableau des Espèces Minerales; par* I. A. H. Lucas. *Seconde partie.* Paris, 1813.] We shall give an account of this book, if possible, in our next Number.

III. *A Treatise on New Philosophical Instruments, for various purposes in the Arts and Sciences, with Experiments on Light and Colours.* By David Brewster, LL.D. F.R.S. E.] We shall notice this excellent work more particularly in a future Number of the Annals.

Part I. of Vol. II. of the Transactions of the Linnæan Society is also published, and will come under our notice in a future Number.

Article X.

Scientific Intelligence; and Notices of Subjects connected with Science.

I. *Method of taking Ironmoulds out of Cotton.*

Every body knows that cottons of all kinds are apt to receive a dirty yellowish, or orange stain, from iron, which, if allowed to remain, gradually corrodes the cloth and forms a hole. At first these stains are easily removed by means of muriatic acid, or any other diluted acid (except vinegar); but, after they have remained for some time, acids have no effect upon them. It may be acceptable to my readers to point out the method of removing these moulds in such inveterate cases.

The iron in them is in the state of red oxide; and it appears, from various facts well known to chemists, that the red oxide of iron has a much greater affinity for cotton cloth than the black oxide. The object in view, therefore, should be to bring the iron in the mould to the state of black oxide; after which, muriatic acid will easily remove it. Now there are two methods of doing this; both of which in the present case answer the purpose completely. The first is to touch the mould with the yellow liquid formed by boiling a mixture of potash and sulphur in water, called hydrogureted sulphuret of potash by chemists. The mould becomes immediately black, and the action of diluted muriatic acid immediately effaces it. The second method is to daub the mould over with ink so as to make it quite black. After this muriatic acid takes it out, as in the former case. I conceive that this is occasioned by the action of the nutgalls in the ink, which reduces the iron in the mould to the state of black oxide.

II. *Composition of Azote.*

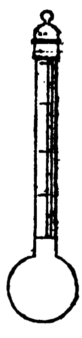
Professor Berzelius has announced, in a letter to a celebrated chemist in London, that he has satisfied himself, by a mode of calculation which he has not explained, that azote is a compound of 44·6 of an unknown inflammable gas, and 55·4 of oxygen gas.

III. *Pure Alumina.*

Mr. Webster lately picked up a very curious mineral upon the beach between Brighthelmstone and Beachy Head. It is a white substance, similar in appearance to a mass of tobacco pipe-clay; but when examined by Dr. Wollaston was found to consist of nothing else than pure alumina. It must have fallen down from the cliff; but its repository was not discovered. It would be well worthy the attention of those Gentlemen who live near the spot to endeavour to find out the position of a bed which would be so interesting in many points of view.

IV. *Atmometer.*

Professor Leslie, of Edinburgh, to whom the scientific world lies under such obligations for his important discoveries on heat, and his differential thermometer, which enables us to detect the changes in temperature that take place in any particular point of space, independent of the general changes which are going on in the place where the observations are made, has lately contrived an instrument for measuring the rate of evaporation, which promises to be of essential service to meteorology, and ought to make one of the instruments of every meteorologist, and to be carefully attended to in every part of the world. Combined with a knowledge of the temperature where the instrument is used, it would furnish us with the means of ascertaining the quantity of vapour that exists in the atmosphere, and thus answer the purpose of a *hygrometer* as well as *atmometer*.



A representation of Mr. Leslie's *atmometer* is given in the margin, and we shall here add such a description of it as will put it in the power of any competent workman to construct it at pleasure. It consists of a thin ball of porous earthen ware, two or three inches in diameter, to which is firmly cemented a long and rather wide tube, bearing divisions, each of them corresponding to a quantity of water which would cover the outer surface of the ball to the thickness of $\frac{1}{1000}$th of an inch. These divisions are numbered downwards, to the extent of 100 or 200, according to the length of the tube. To the top of the tube is fitted a brass cap, having a collar of leather, which, after the cavity has been filled with distilled or boiled water, is screwed tight. The outside of the ball being now wiped dry, the instrument is suspended out of doors, and exposed to the free action of the air.

Evaporation is always proportional to the extent of humid surface. The quantity of evaporation from a wet ball is the same as from an equal plane surface, or from a circle having twice the diameter of the sphere. In the atmometer the humidity transudes through the porous surface, just as fast as it evaporates from the external surface; and this waste is measured by the corresponding descent of the water in the stem. At the same time the tightness of the collar taking off the pressure of the column of liquid, prevents it from oozing so profusely as to drop from the ball; an inconvenience which in the case of very feeble evaporation might otherwise take place. As the process goes on a corresponding portion of air is likewise imbibed by the moisture on the outside; and being introduced into the ball rises in a small stream, to occupy the space deserted by the subsiding of the water in the tube. The rate of evaporation is not affected by the quality of the porous ball, and continues exactly the same when the exhaling surface appears almost dry, as when it glistens with abundant moisture. The exterior watery film attracts moisture from the internal mass with a force inversely as its thickness, and will therefore accommodate the supply to any given degree of expenditure. When this consumption is excessive the water may be allowed to percolate by unscrewing the cap, avoiding, however, the risk of letting it drop from the ball.

V. *Hygrometer.*

None of our readers who have paid any attention to meteorology can be ignorant of the great importance of knowing the quantity of moisture that exists in the atmosphere at any time, and of the numerous contrivances invented for that purpose under the name of *hygrometers.* Of all these there are none that can be accurately compared with each other except the hygrometer of Mr. Leslie, which is merely his differential thermometer having one of the balls covered with a piece of silk. The silk being kept moist, cools the ball by its evaporation; and the degree of cold being proportional to the rapidity of evaporation, that rapidity is marked by the sinking of the liquid in the other tube of the thermometer. Hence it appears that his hygrometer is similar in its indication to his atmometer, indicating only the rate of evaporation. But there can be no doubt that the knowledge of this rate and of the temperature is sufficient to make us acquainted with the quantity of vapour in the atmosphere at the time the observation was made.

Mr. Leslie has lately invented another kind of hygrometer, which, though imperfect in its principle, may, however, be of some utility, and deserves therefore to be known. On that account it will be worth while to give a description of it here. He gives it the name of *hygroscope.* A piece of fine-grained ivory, about an inch and quarter in length, is turned into an elongated spheroid, as thin as possible, weighing only eight or ten grains, but capable of containing, at its greatest expansion, about 300 grains of mercury; and the upper end, which is adapted to the body by means of a delicate screw, has a slender glass tube inserted six or eight inches long, with a bore of nearly the 15th part of an inch in diameter. The instrument being fitted together, its eliptical shell is dipped into distilled water, or lapped round with a wet bit of cambric, and after a considerable interval of time filled with mercury to some convenient point near the bottom of the tube, where is fixed the beginning of the scale. The divisions themselves are ascertained by distinguishing the tube into spaces which correspond each to the thousandth part of the entire cavity, and therefore equal to the measure of about three-tenths of a grain of mercury. The ordinary range of the scale will include 70 of these divisions. To the upper end of the tube is adapted a small ivory cap, which allows the penetration of air, but prevents the escape of mercury, and thereby renders the instrument quite portable.

This hygroscope is largely, though rather slowly, affected by any change in the humidity of the ambient medium. As air becomes drier it attracts a portion of moisture from the shell or bulb of ivory, which, suffering in consequence a contraction, squeezes the mercury so much higher in the tube. But when the air inclines more to dampness, the thin bulb imbibes moisture, and swells proportionally, allowing the quicksilver to subside towards its enlarged cavity; but these variations are very far from corresponding with the real measures of atmospheric dryness and humidity. Near the point of extreme dampness the alterations of the hygroscope are much augmented, but they diminish rapidly as the mercury approaches the upper part of the scale. The contraction of the ivory answering to an equal rise in the dryness of the air is six times greater at the beginning of the scale than at the 70th hygroscopic division, and seems in general to be inversely as the number of hygrometric degrees, reckoning from 20 below. Mr. Leslie, therefore, places another scale along the opposite side of the tube, the space between 0 and 70° of the hygroscope being divided into 100°, and corresponding to the unequal portions from the number 20 to 120 on the logarithmic line.

VI. *Moisture absorbed by various Bodies from the Air.*

Mr. Leslie has ascertained by experiment that 100 grains of the following bodies in the like circumstances absorb the following quantity of moisture from the atmosphere :—

Ivory	7 grains.
Box wood	14
Down	16
Wool	18
Beech	28

VII. *Dryness produced in Air by different Earths.*

Mr. Leslie has ascertained by experiment that when his hygrometer is inclosed in a glass vessel with the following earthy substances, it indicates the following degrees of dryness at the temperature of 60° Fahrenheit:—

Alumina	84°
Carbonate of magnesia	75
Carbonate of lime	70
Silica	40
Carbonate of barytes	32
Carbonate of strontian	23

VIII. *Models of Crystals.*

Mineralogists will learn with pleasure that Mr. Larkin, No. 5, Gee-street, Clarendon-square, Somer's-town, London, has begun to cut models of crystals, according to the system of Haüy, in wood; which he sells at a moderate rate. Complete sets of all the crystals described by Haüy may be had for about 15*l.*; or any part of the series wished for by mineralogists may be had separately. The crystals are remarkable for their beauty, and they are cut with uncommon accuracy. We have no doubt that as soon as Mr. Larkin's models of crystals become generally known every mineralogist will furnish himself with a set.

Article XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 29th of April the paper by Berzelius and Marcet on the alcohol of sulphur was continued. They obtained this substance by subliming sulphur through red-hot charcoal in a porcelaine tube, and receiving the product in water. Thus obtained it was usually of a yellow colour, from an excess of sulphur which it contained; but it was reduced to a state of purity by distilling it in a glass retort.

Thus obtained it was a colourless liquid, like water, of a pungent disagreeable taste, and a stronger smell than sulphureted hydrogen gas. It boiled at the temperature of 110° or 115°; and at the temperature of 53°, when the barometer stood at 30 inches, it was capable of furnishing a vapour which supported a column of mercury 7¼ inches high; so that when mixed with air at the common temperature it increases its bulk one-fourth. It is more volatile than ether, and produced so much cold during its evaporation that mercury was frozen. The alcohol of sulphur may be cooled down to —50° without congealing. It dissolves in alcohol and ether, depositing at the same time its excess of sulphur, if it happen to contain any. It readily dissolves sulphur. Mercury may be boiled in it without any alteration. Potassium, when heated with it, undergoes no change; but when heated in an exhausted retort filled with the vapour of alcohol of sulphur it burns with a red colour, a black matter covers its surface, and on admitting water a solution of hepar sulphuris is formed, mixed with charcoal.

To determine if it contained any hydrogen, its vapour was mixed with dry oxygen gas, and detonated by electricity. No water was obtained. Oxymuriatic acid gas was made to pass through it for an hour and a half, and then through water; but no muriatic acid made its appearance, as would have been the case if hydrogen had been present in the alcohol of sulphur. It was made to pass through red-hot muriate of silver; but none of the silver was reduced to the metallic state, as would have been the case if hydrogen had been present. Finally, it was made to pass over several metallic peroxides at a red heat (as red oxide of iron, black oxide of manganese). The oxides were reduced, and converted to sulphurets; but no moisture was deposited in the tube, though surrounded with ice. From all these trials it appears that alcohol of sulphur contains no hydrogen.

On May the 6th the remainder of the paper by Berzelius and Marcet on the alcohol of sulphur was read. The next object was to ascertain the presence of carbon in this oily substance. When burnt in oxygen gas the residual gas was found to contain sulphurous acid gas. This being removed, some carbonic acid gas remained, which rendered lime-water turbid, and changed pure lime into the carbonate of lime. Both these acid gases being removed, a combustible gas remained, which detonated when mixed with oxygen gas, and was converted into carbonic acid. It was, therefore, carbonic oxide. Alcohol of sulphur being mixed with a caustic ley, with barytes-water, and with lime-water, was slowly decomposed, and a quantity of carbonic acid formed. From these, and several other experiments of a similar nature, it follows demonstrably that the alcohol of sulphur contains carbon. It is in fact a compound of carbon and sulphur, and may therefore with propriety be called *sulphuret of carbon.*

The last object was to determine the proportion of carbon present in this compound. A great variety of methods were tried, such as burning in oxygen gas, decomposition in alkalies, &c.; but none of them were found to answer. At last they succeeded, by passing the sulphuret of carbon very slowly through a red-hot tube filled with red oxide of iron. The gaseous products were received over mercury. The red oxide of iron was partly converted into sulphuret. To determine the quantity of sulphur present it was dissolved in nitromuriatic acid, and the whole sulphur converted into sulphuric acid. This acid was thrown down by barytes, and its quantity accurately ascertained. The gases over mercury were found to be a mixture of sulphurous acid and carbonic acid. The sulphurous acid was absorbed by brown oxide of lead, which by that means was converted into sulphate; and the additional weight being ascertained, determined the proportion of sulphurous acid present in the gas. The carbonic acid was absorbed by potash, and its weight determined in the same manner. From these data it was possible to determine the proportion of sulphur and carbon present in the alcohol of sulphur. The result was that it is a compound of

Sulphur	84·83
Carbon	15·17
	100

or of two atoms of sulphur and one of carbon.

On Thursday the 13th of May an appendix to the preceding paper, by Professor Berzelius, was read. It consisted of the four following particulars :—

1. An account of the method employed in determining the proportions of carbon and sulphur in the sulphuret of carbon. The mode was to decompose a given weight of sulphuret of carbon by passing it through red-hot peroxide of iron, and receiving the products over mercury. The sulphuret of iron formed was dissolved, and the sulphur converted into sulphuric acid. The weight of sulphuric acid, of sulphurous acid, and of carbonic acid, formed, was ascertained; and from the known composition of these three substances the proportion of carbon and sulphur was determined. Two experiments were made. In the first the loss amounted to $\frac{1}{3}$d of a per cent.; in the second to eight thousandth parts.

2 Some observations on the atomic theory. According to Mr. Dalton's theory, sulphuret of carbon is a compound of two atoms of sulphur and one of carbon. Professor Berzelius makes

some remarks upon Sir H. Davy's numbers, which he has adopted in his Elements of Chemistry, and shows that they do not answer for the metallic sulphurets with the requisite simplicity. Yet if any sulphuret be treated with an acid so as to convert the metal into an oxide, the quantity of hydrogen disengaged will always indicate exactly the quantity of oxygen in the water decomposed, which would be sufficient exactly to acidify the sulphur. Berzelius thinks that unit ought to be employed to indicate an atom of oxygen, and that the weight of the other atoms should be determined by the proportion in which they combine with oxygen.

3. On the combination of sulphuret of carbon with bases. Berzelius found that sulphuret of carbon combines with ammonia and with lime, the only bases tried. These combinations he calls *carbo-sulphurets*. Carbo-sulphuret of ammonia is formed by putting sulphuret of carbon into a tube, and letting up into it ammoniacal gas as long as it will absorb it. A yellow pulverulent substance is formed, which sublimes unaltered in close vessels, but so deliquescent that it cannot be passed from one vessel to another without absorbing moisture. If it be heated in that state crystals of hydrosulphuret of ammonia make their appearance. Carbosulphuret of lime is formed by heating some quicklime in a tube, and causing sulphureted carbon to pass through it. The lime becomes incandescent at the time of the combination. On the outside there is formed some sulphuret of lime, which gives it a yellow colour. This formation is owing to the action of the air, and is merely superficial.

4. When sulphuret of carbon is left for some weeks in contact with nitromuriatic acid, it is converted into a substance having very much the appearance and physical properties of camphor; being soluble in alcohol and oils, and insoluble in water. This substance Berzelius found to be a triple acid, composed of two atoms of muriatic acid, one atom of sulphurous acid, and one atom of carbonic acid. He proposes to call it *acidum muriatico-sulphuroso-carbonicum*.

On Thursday the 20th of May a paper by Dr. Reid Clanney, of Sunderland, was read, on a lamp for preventing explosions in coal-mines by the combustion of carbureted hydrogen gas. Dr. Clanney began by giving an historical account of the accidents of this nature which have taken place in the neighbourhood of Sunderland within the last seven years; from which it appears that above 200 workmen have been suddenly killed, and more than 300 women and children left in destitute circumstances by these dreadful explosions. His lamp is extremely simple. It consists of a kind of lantern made air tight; in which a candle is kept burning. Air is constantly blown into it through water by a pair of bellows to support the combustion, and allowed to escape in the same manner through a valve. By this means no more air can explode than what is within the lantern. Thus no accident can ever happen, and the workmen will be sufficiently warned to make their escape in time.

I cannot conclude this account without mentioning two circumstances which would deserve a more accurate investigation than they have yet received. I never heard of any accident from fire-damp in any of the coal-mines in Scotland. The accidents, I am told, are much more frequent in Staffordshire than about Newcastle. Do these differences depend upon the nature of the coal, or on the mode of working the mine?

LINNÆAN SOCIETY.

May the 4th the remainder of Mr. Anderson's paper on different species of rubus was read. He terminated it with a list of various rare plants which he had observed in Britain, especially in Scotland.

Some quadrupeds from North America were exhibited to the Society by Lord Stanley.

May the 24th, at the Annual General Meeting, the following Officers were elected :—

JAMES EDWARD SMITH, M.D. President.
THOMAS MARSHAM, Esq. Treasurer.
ALEXANDER MACLEAY, Esq. Secretary.
MR. RICHARD TAYLOR, Under Secretary.

The five following Gentlemen were chosen into the Council:

John Barrow, Esq.
Sir Thomas Gery Cullum, Bart.
Philip Derbishire, Esq.
Mr. James Dickson.
Edward Lord Stanley.

In the room of the five following Gentlemen:—

Henry Ellis, Esq.
Thomas Furly Forster, Esq.
Lieut.-Col. Thomas Hardwicke.
Claude Scott, Esq.
George Viscount Valentia.

Since the last General Meeting about four British and three foreign members have died, and 34 new members have been elected; so that the number of fellows at present amounts to 437; the foreign members to 64, and the associates to 40.

Article XII.

New Patents.

John Heathcoat, of Loughborough; for certain improvements and additions to a machine for the making or manufacturing of bobbin lace, or lace nearly resembling foreign lace. Dated March 29, 1813.

John Bennet, of Bristol; for a metal dovetail joint, applicable to portable and other furniture, and any kind of framework requiring strength and durability, and to many useful purposes. Dated April 7, 1813.

David Thomas, of Bristol; for a new and improved method of burning animal bones for the purpose of extracting the grease or fat property therefrom, and likewise for extracting the spirituous quality thereform, and for reducing the remainder, or dry parts of bones, into a substance sufficiently prepared for being ground into ivory black. Dated March 30, 1813.

Joseph Egg, London; for a method of applying and improving locks. Dated March 30, 1813.

Robert Hall and Samuel Hall, of Basford; for a machine for dressing, getting up, or furnishing frame-work knitted goods from the stocking frame, whether consisting of hose, socks, caps, mitts, gloves, or of any other kind or description whatever, and whether made of cotton, lamb's wool, Vigonia wool, silk, mohair, or any other vegetable or animal substance whatever, or any intermixture of these substances one with another. Dated March 30, 1813.

James Timmins, of Birmingham; for an improved method of making and erecting hot-houses, and all horticultural buildings, and also the making of pine pits, cucumber lights, sashes, and church windows. Dated April 7, 1813.

John Rangeley, of Oakwell-hall, near Leeds; for an improved method of constructing and working engines or machines for lifting or raising of weights, turning of machinery of all descriptions, drawing carriages on railways, and capable of being applied to all purposes where mechanical power is required. Dated April 13, 1813.

Robert Lewis, of Birmingham; for a method of making brass (or any other metal of which the component parts are copper and zinc) chimney-pieces, or chimney-piece frames, plain or ornamented, either cast or of rolled metal mounted on any other substance, or of which the outward mouldings, or frame and inward pilasters, shall be composed of such metal. Dated April 13, 1813.

Charles Plinth, London; for various improvements in the construction of a vessel, machine, cylinder, reservoir, or fountain (which he calls "The Regency Portable Fountain"), used in the manufacture of water simply impregnated with fixed air or carbonic acid, and of artificial mineral and soda waters, and in the delivery of the same therefrom; and also in the delivery of cyder, perry, and other liquids. Communicated to him by certain foreigners residing abroad. Dated April 13, 1813.

Robert Campion, of Whitby, for a new and improved method of making and manufacturing double canvas and sail-cloth with hemp and flax, or either of them, without any starch whatever. Dated April 13, 1813.

Charles Augustus Busby, London; for certain methods of constructing locks of canals, docks, and navigations. Dated April 14, 1813.

Thomas Mead, of Sculcoats, Yorkshire; for an endless chain of a peculiar construction, with appendages, which, with the assistance of other mechanical apparatus, is applicable to a variety of useful purposes. Dated April 28, 1813.

Article XIII.

Scientific Books in hand, or in the Press.

Mr. Leslie, Professor of Mathematics in the University of Edinburgh, is just upon the point of publishing A View of Experiments and Instruments depending on the relation of Air to Heat and Moisture.

Capt. Laskey has in the Press a Scientific Description of the Rarities in that magnificent collection the Hunterian Museum, now deposited at the College of Glasgow. It is intended to comprise the rare, curious, and valuable articles, in every department of Art, Science, and Literature, contained in that great repository. This work may be expected to appear early in July.

Mr. Henry Alexander, surgeon, will shortly publish a Comparative View of the different Modes of operating for the Cataract.

A second Volume of M. de Luc's Geological Travels on the Continent is nearly ready for publication.

Dr. Bateman is preparing a Practical Synopsis of Cutaneous Diseases, according to the Arrangement of Dr. Willan, and exhibiting a concise View of the Diognostic Symptoms, and the Method of Treatment.

Mr. Mawe, Author of Travels in the Brazils, is about to publish a Treatise on Diamonds, and other Precious Stones, including their History natural and commercial.

*** *Early Communications for this Department of our Journal will be thankfully received.*

Article XIV.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Min.	Med.	Thermometer. Max.	Min.	Med.	Evap.	Rain.	
4th Mo.										
April 24		30·14	29·95	30·045	—	35	—	—		
25		29·95	29·71	29·830	53	43	48·0	—	·51	
26		29·71	29·51	29·610	—	39	—	—		
27		29·51	29·36	29·435	48	45	46·5	—	·52	
28		29·61	29·36	29·485	50	41	45·5	0·15	4	
29		29·65	29·61	29·630	49	39	44·0	—	·21	
30		29·65	29·63	29·640	—	—		—	—	●
5th Mo.										
May 1	N E	29·72	29·63	29·675	59	45	52·0	—	·25	
2	Var.	29·79	29·72	29·755	61	46	53·5	—		
3	E	29·79	29·73	29·760	66	50	58·0	0·17	·17	
4	Var.	29·86	29·73	29·795	65	51	58·0	—	·24	
5	N W	29·91	29·87	29·890	64	49	56·5	—	—	
6	N E	29·87	29·69	29·780	68	50	59·0	—	·16	
7	S E	29·70	29·69	29·695	69	45	57·0	—	8	☾
8	S E	29·70	29·57	29·635	68	49	58·5	0·27	—	
9	N	29·73	29·57	29·650	72	52	62·0	—	—	
10	Var.	29·90	29·73	29·815	69	47	58·0	—	8	
11	Var.	29·73	29·60	29·665	67	53	60·0	—	5	
12	S W	29·64	29·60	29·620	74	49	61·5	—	3	
13	S W	29·64	29·46	29·550	72	51	61·5	0·35	—	
14	Var.	29·47	29·39	29·430	68	52	60·0	—	9	
15	S W	29·70	29·57	29·635	65	47	56·0	—	—	○
16	S W	29·72	29·41	29·565	65	49	57·0	—	·51	
17	N W	29·82	29·68	29·750	60	47	53·5	—	—	
18	Var.	29·80	29·78	29·790	60	49	54·5	0·45	·25	
19	S W	29·84	29·56	29·700	69	50	59·5	—	—	
20	W	29·59	29·57	29·580	61	42	51·5	—	—	
21	W	29·64	29·59	29·615	59	39	49·0	—	—	
22	W	29·69	29·59	29·655	58	30	44·0	0·28	·53	
		30·14	29·36	29·678	74	30	54·79	1·67	3·72	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fourth Month. 24. Heavy *Cumulostratus* clouds throughout the day. 25. Rain nearly the whole day. 26. Max. Temp. at 9 a. m. Cloudy: clear, with *Cirri*, at evening. 27. A wet day. 28. Wet morning: cloudy. 29, 30. Cloudy: much wind.

Fifth Month. 1. Max. temp. at 9 a. m.: wet. 2. Cloudy, a. m.: but p. m. the sky cleared pretty suddenly: some dense *Cumulus* clouds remaining in the N. E. to the summit of one of which a *Cirrostratus* was observed for a considerable time adhering; which was at length incorporated with the larger cloud. The moon appeared with a pale golden crescent, the remainder of the disk being pretty conspicuous. 3. Dense *Cumuli* to the S. with *Cirrus* and *Cirrocumulus* intermixed, as usual before thunder: a shower of large drops about sunset. 4. Overcast, a. m. About 6 p. m. (after some previous dripping) a thunder storm, the weight of which fell to the E. of us. A most brilliant and perfect *bow* was now exhibited for about 40 minutes. A nightingale continued to sing with spirit in the midst of the shower. 6. a. m. Much dew: p. m. a large thunder cloud in the N. and which moved by W. to S. after which a storm in that direction, nearly out of hearing, till midnight: then—sudden heavy rain. 7 a. m. Cloudy: p. m. (after a shower) clearer, but with indications of more rain. 8. An appearance of much electrical action in the clouds, far to the S. and S.W. 9. A few drops of rain, a. m. various modifications of cloud. 10. *Nimbi*: dripping afternoon: rainbow: fine evening. 13. Cloudy; windy. 14. Much wind. 15. The same: calm night. 16, 17, 18. Much wind: showers.

RESULTS.

Winds variable.

Barometer: greatest height..........30·14 inches;
Least..................29·36 inches;
Mean of the period....29·678 inches.

Thermometer: greatest height.......74°
Least..............30°
Mean of the period....54·79°

Evaporation (at the surface of the earth) 1·67 inches.

Rain, 3·72 inches.

TOTTENHAM, L. HOWARD.
Fifth Month, 26, 1813.

ERRATA in our last Number.

Page 385, line 18, *for* "car" *read* "cor."
Ibid. line 38, *for* "Annesley" *read* "Anderson."

END OF VOL. I.

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